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NOTES ON THE HARDENING AND TEMPERING OF EUTECTOID CARBON STEEL AND ON THE SHORE TEST.

By Henry M. Howe and Arthur G. Levy.

SUMMARY.

Part I.—If eutectoid steel is heated to 800° C., and quenched either immediately or after slow cooling to one or another of a descending series of temperatures, the volume of the quenched steel decreases progressively as the quenching temperature sinks towards the recalescence or Ar1 (Section 5), showing that the arrest of the transformation by the quenching becomes progressively less complete, whereas little change in the resultant hardness occurs (Sections 6 to 10). As the quenching temperature is lowered from above to below Ar1 the decrease of volume is greatly and suddenly exaggerated (Section 12), and a sudden loss of hardness occurs (Section 13). Further lowering of the quenching temperature does not affect the volume (Section 12), showing that the transformation completes itself on passing Ar1; but the hardness decreases progressively, in our belief because of the increased opportunity for the coalescence of the cementite and ferrite respectively (Sections 13 to 15).

Part II.—The rate of decrease of the hardness of hardened eutectoid steel as the time and temperature of tempering increase is shown graphically (Sections 16 to 20). The effect for temperatures above 400° C. we refer to coalescence, and to this also may be due an important part of the time effect for lower temperatures (Section 21).

The microstructural changes caused by variations in quenching temperature and by tempering are illustrated and discussed (Sections 11 and 22). Those caused by tempering are obscure, and so far as we now see are even less usefully indicative than the fracture (Section 23).

The oxide colors found in the tempering cracks are the deeper the longer the tempering at given temperature (Section 23 *a*).

Part III.—The observed Shore hardness increases with the thickness of the specimen (Sections 24 to 26), and with the distance from the edge of the specimen (Section 27), within limits, and also on superposing well-fitting specimens, but not when many specimens are set on one another, probably here because of the cumulative effect of surface inaccuracies (Section 28). What thickness and distance from the edge are required for true readings we do not know, but neither a thickness of 0.62 in. in hardened eutectoid steel nor a distance of 0.078 in. from the edge in tempered steel of about 0.20 per cent of carbon suffices.

On repeating the Shore test in the very same spot, the observed hardness at first increases rapidly, and thus becomes about tripled, so that that of dead soft ferrite equals that of hardened eutectoid steel (Section 29). But after numerous repetitions the observed hardness drops abruptly, often apparently to zero, to recover to or above the previous maximum. This recovery may be spread out over as many as twelve successive impacts (Sections 30 and 33).

Increasing frequency of test, though its effect is obscured by these abrupt drops, seems to accelerate the increase of hardness slightly (Sections 31 and 34).

The Shore hardness of such a conglomerate as unhardened eutectoid steel may on one hand be extremely uniform, or on the other hand it may vary greatly, with a surprising tendency to be concentrated on two numbers about midway of the range, and to avoid the numbers intermediate between these two (Figs. 8 and 9 and Section 35).

NOTES ON THE HARDENING AND TEMPERING OF EUTECTOID CARBON STEEL AND ON THE SHORE TEST.

By Henry M. Howe and Arthur G. Levy.

1. *Introduction.*—This paper gives and tries to explain our results showing the influence of the quenching temperature, and of the temperature and time of tempering, on the hardness and on the microstructure of quenched (hardened) steel containing 0.92 per cent of carbon. It records, further, certain preliminary studies of the Shore scleroscope test. Eutectoid steel was selected because its nature is the simplest, avoiding the complicating effects of the pro-eutectoid ferrite and cementite of lower and higher-carbon steel, respectively.

Our Brinell hardness for the hardened and annealed states agrees with that recorded by Brinell for such steel,[1] and our Shore hardness for the annealed state agrees with that of Shore,[2] but our Shore hardness for the hardened state, about 65, is far less than that recorded by Shore, which is about 106, probably because of the change made in his scale since our present instrument was made.

2. *Material.*—Our experiments were with the eutectoid steel referred to in our previous works. Its chemical composition is as follows:

Carbon	0.92	per cent
Silicon	0.14	"
Manganese	0.123	"
Phosphorus	0.009	"
Sulfur	0.011	"

3. *Definitions.—To harden steel* is to cool it rapidly, for instance by quenching it in water from above the bottom of the transformation range, commonly called the critical range, that is, from above the recalescence or A1.

[1] Wahlberg, *Journal*, Iron and Steel Institute, No. 1, p. 267 (1901).

[2] "The Shore Scleroscope," Shore Instrument and Manufacturing Co., New York, 1912, p. 5.

To temper hardened steel is to reheat it to any temperature below A1. We do not try to set the limit between tempering and annealing.

The tempering temperature is that to which the hardened steel is thus reheated.

The tempering time is that during which the steel is held at the tempering temperature. Under many conditions, especially in the tempering of thick, and hence slowly heating, objects, tempering goes on to an important degree during the latter part of the time spent in reaching the tempering temperature. In our experiments this effect may be neglected, because the thin pieces used were brought quickly to their several tempering temperatures.

PART I. INFLUENCE OF VARIATIONS IN QUENCHING TEMPERATURE.

4. *Experimental Conditions.*—(See Figs. 1 and 2 and Table I.) Wads of this steel, $\frac{5}{8}$ in. in diameter and $\frac{5}{8}$ in. thick, were heated side by side in 62 minutes to 797° C., while lying in a horizontal wire-wound tubular furnace $2\frac{1}{2}$ in. in diameter and 24 in. long, with a variation of less than 5° C. in the part occupied. After remaining at 797° for 5 minutes one wad was drawn and quenched immediately. The furnace with the remaining wads was then cooled slowly, at the rate indicated in Fig. 1. That is to say, the temperature fell in 22 minutes to 724°, and was then held there for 5 minutes, when a second wad was drawn and quenched. The furnace and the remaining wads were then further cooled as indicated, one wad being drawn and quenched at 700°, one at 670°, and the last cooled slowly with the furnace to room temperature (Fig. 1). The 750° wad was treated separately (Fig. 2). Decarburization was lessened by setting an abundance of charcoal within the tube of the furnace, and thus giving it an atmosphere chiefly of carbonic oxide.[1] Approxi-

[1] Under these conditions the atmosphere of the furnace consists chiefly of nitrogen and carbonic oxide, as is shown by the strong tongue of flame which bursts with a sharp pop from the end of the furnace when it is opened, and by the extreme sharpness of all the surface details of the wads even after a rather long high heating. We question whether the decarburization is greater under these conditions than when a salt bath is used, for we find that heating even in a salt bath initially free from iron oxide causes some surface decarburization.

mately $\frac{1}{64}$ in. was ground away from all surfaces of each wad so as to remove any part which was decarburized in spite of this precaution. The density was next determined by weighing in air and in water, and the results reduced to a temperature of 15° C. for both the metal and the water. After this the Brinell hardness was determined at one end of each wad in the usual way, with a 10-mm. ball and a load of 3000 kg. applied for $\frac{1}{2}$ minute. The surface of one end was then ground while

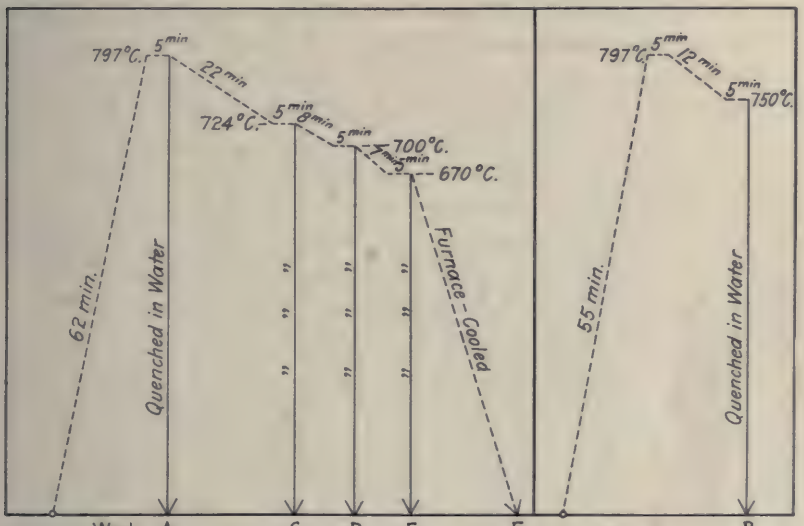

FIG. 1.

FIG. 2.

FIGS. 1 and 2.—The Thermal Treatment of the Authors' Specimens of Eutectoid Steel of Table I.

drenched with water, polished, etched and photographed; the Shore hardness was determined, and then the Brinell hardness was redetermined at several spots. Finally, the Shore hardness of the other end also of each wad was determined after polishing and etching.

At the rate of cooling of these wads while within the furnace the recalescence, Ar1, lies at about 720 to 724° C., so that wads *A* and *B* were quenched from above, and *D* and *E* from below Ar1. Wad *C* seems to have been quenched at just above Ar1.

TABLE I.—INFLUENCE OF THE QUENCHING TEMPERATURE ON THE HARDNESS AND THE DENSITY OF EUTECTOID STEEL.

Quenching Temperature, deg. Cent.	Wad Letter.	The Authors' Results: Carbon, 0.92 per cent.																		Langley: Carbon, 0.871 per cent.		Oknof: Carbon, 0.93 per cent.	
		Brinell Hardness Number.			Shore Hardness Number.																		
		End A.			End B.																		
					Central Dark Area.					White Hard Ring.					Outer Dark Ring.								
		Central Dark Area.	Outer White Area.	Intermediate Border.	No. of Tests.	Maximum.	Minimum.	Average.	Range.	No. of Tests.	Maximum.	Minimum.	Average.	Range.	No. of Tests.	Maximum.	Minimum.	Average.	Range.	Specific Gravity at 15°/15° C.	Specific Volume at 15°/15° C.	Difference between Successive Measurements of Specific Volume in units of 0.00001.	Specific Volume.	Difference between Successive Measurements of Specific Volume, in units of 0.00001.	Specific Volume at 0°/4° C.	Difference between Successive Measurements of Specific Volume, in units of 0.00001.
1200.....	..	Average: 640 [a, b]			Average: 66 [a, b]															....	....		0.12900		....	
1100.....	*G*																			....	....		0.12895	+5	....	
1000.....	..																			....	....		0.12890	+5	0.12892	
900.....	..																			....	....		0.12865	+25	0.12895	−3
800 (797).....	*A*	555	653, 653, 590, 590		5	56	55	55.2	1	11	68	62	64.8	6	4	56	56	56	0	7.8049	0.12812		....	+28	0.12886	+9
750.....	*B*	653, 653, 653, 627, 627[b]			7	56	54	54.9	2	7	62	60	61.0	2	8	57	54	55.2	3	7.8095	0.12805	+7	0.12837		0.12883	+3
724.....	*C*	341, 387	578	429, 555	7	53	52	52.8	1	7	63	62	62.1	1	8	57	53	55.5	4	7.8191	0.12789	+16	....		....	+77
700.....	*D*	183[b]								17	27	25	26.1	2[b]						7.8449	0.12747	+42	....		0.12806	
670.....	*E*	175[b]								20	27	24	25.7	3[b]						7.8449	0.12747	0	....	+46	....	+8
650.....	..																			....	....		....		0.12798	−3
600.....	..																			....	....	+1	....		0.12801	+2
400.....	..																			....	....		....		0.12799	0
Annealed or Furnace-cooled	*F*	166[b]								16	28	22	25.2	6[b]						7.8458	0.12746		0.12791		0.12799	

[a] This number represents measurements on a wad $\frac{1}{4}$ in. thick, whereas all others represent those made on wads $\frac{5}{8}$ in. thick.

[b] Wads *D*, *E*, *F* and *G*, and end *A* of wad *B*, lacked this variation in etching tint; hence each was tested as a whole.

5. *The Results: Quenching Temperatures above the Transformation Range; Volume Changes.*—Though a full discussion of the volume changes would lead us too far, we may point out that the progressive loss of volume as the quenching temperature is lowered from case to case, from 797 to 700° C., or below the recalescence, shows that the degree to which the transformation from the high to the low-temperature state occurs, despite the quenching, increases progressively as the quenching temperature thus falls. One of us[1] has explained that this is due jointly (1) to the flatter thermal gradient which exists within the piece when the quenching temperature is low; (2) to the consequent lessening of the internal pressure and hence of the resistance to the transformation; and (3) to the decrease of the quenching stresses. Further, that this progressive increase of the degree of transformation with falling quenching temperature need not be accompanied by a progressive loss of hardness, as might at first be expected, because though the second part of the transformation is from the harder martensite towards the softer pearlite through troosite, yet the first part is from softer austenite to the harder martensite, so that an increase of the degree to which the transformation occurs would lead to an increase or a decrease of hardness according to whether the hardening, representing an increase in the ratio of martensite to austenite, outweighs or is outweighed by the simultaneous softening representing an increase in the ratio of irresoluble ferrite and cementite to martensite.

A fourth cause—decrease of the diffusion of the carbon—does not apply to the present case, because here the temperature reached in the heating up is the same in all cases. Indeed the opportunity for diffusion is greater for the lower-temperature quenchings, because of the greater time available in cooling down to them, than for quenchings at the higher temperatures.

This progressive loss of volume as the quenching temperature falls, and hence as the degree to which the transformation occurs despite the quenching, clearly reflects the smaller volume of the mixture of ferrite plus cementite than of martensite, familiar through the greater volume and smaller density of

[1] "Why does Lag Increase with the Temperature from which Cooling Starts?", Howe, *Transactions*, Am. Inst. Min. Engrs., Vol. 45, p. 516 (1913).

hardened than of annealed steel. Thus Charpy and Grenet[1] found that the coefficient of dilatation of hardened steel was appreciably less than that of like steel when annealed, whence we infer that, as the hardened steel is heated and the arrested transformation is thus enabled to proceed, the progressive contraction which this causes, as it goes on step by step, counteracts to a certain degree the thermal expansion representing the rise of temperature, and thus leads to a lower apparent coefficient of expansion.

6. *Hardness Changes: Irregularities of Hardness.*[2]—Each of the three wads quenched from less than 100° above the recalescence had a distinct dark-etching irregular central area, materially softer than the outer and lighter-etching part.[3] The shape of these areas and the shape, position, and hardness number of the Brinell impressions in the 724° C. wad are shown in Fig. *E*, Pl. I.

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[1] *Bulletin*, Soc. d'Encouragement, 1903, No. 2, pp. 488–491.

[2] Brayshaw (*Proceedings*, Institute of Mechanical Engineers, April, 1910, p. 557) shows great differences in the hardness of different parts of a quenched steel bar, and in particular that the end which enters the water first may be very much harder than the other, the Brinell hardness of the front end being in one case 713 against 444 at the rear end. By a "Brinell" and a "Shore" unit we refer to a difference of 1 in the unit place in the arbitrary numbers by which these results are expressed, so that the difference between 66 and 64 of the Shore scale is 2 Shore units.

[3] Hanemann has recorded this concentric banding of harder martensitic and softer troostitic rings in hardened steel, referring it reasonably to the expansion in passing to the state of martensite and the contraction in passing beyond to that of troostite, with probably much undetectable ferrite and cementite. ("Einführung in die Metallographie und Wärmebehandlung," p. 90, 1915, Borntraeger, Berlin). He finds, for instance, a 5-mm. (0.197-in.) outer shell of martensite; next a layer of troostite; then martensite; then troostite again; and finally in the very center single martensite specks.

Were the degree to which the transformation occurs determined solely by the rate of cooling, then we should have the most martensite in the fastest cooling outer skin and progressively more troostite as we approach the slowest cooling center. But because martensite is so much bulkier than troostite, pressure as such makes for troostitization, and tension for martensitization. The tension caused by the formation of troostite in the second or subcutaneous layer thus leads to a second formation of martensite, which arrests itself by means of the expansion and pressure which it creates, and thus is followed by a second troostite layer, and so forth. Clearly, the exact position and number of these successive rings may vary greatly with the thickness and composition of the piece quenched.

In general harmony with these observations is that of Benedicks (*Journal*, Iron and Steel Inst., 1908, No. II, p. 243), that in hardened steel of say 1.6 per cent of carbon, that is, one in which austenite may be formed by rapid quenching from a high temperature, austenite is lacking entirely in the outer layer, because there the pressure necessary for its retention in a pure carbon steel is lacking. That is to say, in such a steel the tendency of the austenite to transform is so great, because it lacks the restraint of the impeding elements, manganese and nickel, that this transformation goes on as far as the martensite stage, unless to the restraining action of the rapid cooling is added that of pressure, to resist the expansion incident to the change from austenite to martensite.

Note that the hardness, 578, where the impression is wholly in the white, is 70 per cent greater than where it is wholly in the dark, 341; and that it increases to 429 where it is about half in the white and to 555 where it is chiefly but not wholly in the white, so that the hardness in each case is roughly proportional to the proportion of white area included in the impression.

The ends of the cylindrical wads on which these roughly concentric areas exist should be recognized clearly as forming part of the true exterior of the wads, quite as much as the cylindrical sides do. We find like indications of such soft dark spots on these sides also, but we have not explored them.

The shape and etching contrast of these soft spots vary greatly from wad to wad, and from part to part of a given wad. Thus the opposite ends of each of these three wads when etched had a dark irregular soft central area, surrounded by a hard white ring, outside of which in turn lay a darker bluish or brownish ring, approximately of the hardness of the central soft area, as shown in Table I. A martensitic shell outside of the troostitic ring would naturally be expected, but if there was any in these cases it was too narrow to be detected with confidence.

Moreover, though for the 800° quenched wad the deviations of Shore and of Brinell hardness are nearly alike, about 20 per cent of the minimum value, the Shore deviation at end *B* of the 724° quenched specimen is far less than the Brinell deviation at the end *A*—about 20 per cent as against 70 per cent.

7. *Cause of the Deviations in Hardness in a Given Wad.*—This difference in etching tint and hardness is not due to a corresponding difference in carbon content, as is proved by the complete absence of any corresponding variation in either etching tint or in hardness in these same three wads after annealing. Thus, 51 determinations of the Shore hardness of these three specimens after annealing by heating them to 730° for 5 minutes, furnace-cooling them to 450°, and thence air-cooling them, lay, with two exceptions, between 23 and 26, and thus within the usual limits of observational error for a uniform material. So, too, with the three unhardened wads, those quenched after cooling to 700° and to 670° and the furnace-cooled one, of Table I.

That the deviations of hardness of the hardened specimens

are not due to a fault of either the instrument or the observer is shown, not only by the uniformity of the hardness determinations of these unhardened specimens, but also by that of four other materials, as shown in Table II.

Hence the deviations may be referred to corresponding local deviations in the hardening conditions, first in the position and rapidity of the convection currents of steam, and second, in the stresses induced both by the aeoliotachic or uneven rates of cooling, and by the expansion of the transformation from austenite to martensite and the contraction in passing beyond to troostite and sorbite. The irregularity of the hardness and other physical properties of hardened and untempered

TABLE II.—UNIFORMITY OF SHORE HARDNESS OF FOUR HOMOGENEOUS METALS.

Metal.	Thickness, in.	Shore Hardness Number.			
		Number of Tests.	Maximum.	Minimum.	Range.
Sheet silver, annealed	$\frac{1}{16}$	26	$6\frac{1}{2}$	$5\frac{1}{2}$	1
Lake Superior copper, rolled	$\frac{3}{8}$	19	$13\frac{1}{2}$	$11\frac{1}{2}$	2
Ultra-low-carbon steel of American Rolling Mill Co.	1	24	26	25	1
Hadfield's austenitic manganese steel	$\frac{1}{2}$	18	31	29	2

NOTE.—These materials are described in a note to Section 29.

or only partly tempered high-carbon-steel pieces, especially of those from which the outer part has not been removed, is familiar. With our present purpose of studying the surface hardness we cannot use the common device of lessening this irregularity by removing the outer parts of the specimen. The great variations in hardness in a single quenched bar of so very expert a hardener as Brayshaw[1] are instructive.

Here we call attention to the striking fact that, just as the line of demarcation between troostite and martensite in any given quenched and untempered specimen is usually very sharp, with an abrupt change in etching tint from a nearly uniform lighter martensite to a nearly uniform darker troostite, so here

[1] *Loc. cit.*

there is a sharp change in hardness. For instance, the Shore hardness determinations for the hardened wads of Table VIII fall into two distinct classes, a softer and a harder one. The numbers in each of these classes for any given wad differ among themselves by at most 3 Shore units, with a single exception, but with two exceptions the hardest of the soft class in any given wad is from 7 to 10 units softer than the softest of the hard class. These determinations are discussed more fully in Section 25.

8. *The Nature of Troostite.*—The abruptness of the change in both etching tint and hardness does not harmonize readily with our current conceptions of the nature of troostite. This conception is that, just as the loss of hardness brought about by reheating hardened steel to successively higher temperatures is strictly continuous, as shown in Fig. 3, so between the fully hardened martensite at one end of the series, and the fully transformed but irresoluble mixture of ferrite and cementite called sorbite reached at 400° C., there is a strictly continuous series of intermediate transformation products made up of progressively varying proportions of martensite on one hand and irresoluble ferrite and cementite on the other hand. We see no reason to doubt this of the products formed by reheating hardened steel. But if it were true also of the different parts of a wad of imperfectly hardened but untempered steel, then between those different parts there should be a strictly continuous change in etching tint and in hardness, instead of the abrupt change which here occurs along an irregular internal surface. We call attention to this without now trying to throw further light on it. It suggests that the martensite-troostite series in imperfectly hardened but untempered steel is not identical with that brought about by tempering thoroughly hardened steel.

9. *Effect of Quenching Temperature on Hardness.*—(See Table I.) The Brinell hardness data do not distinguish sharply enough between the hard and soft parts to make their indications trustworthy. The far more searching Shore results show in each of the three areas, the "central dark," the "white hard ring," and the "outer dark ring," a slight decrease of hardness, of at most 2.7 Shore units, with decrease of quenching

TABLE III.—RELATION OF BRINELL AND SHORE HARDNESS TO THE QUENCHING TEMPERATURE.

Specimen No.	Observer.	Carbon, per cent.	Method.	Hardness Numbers at Various Quenching Temperatures, deg. Cent.																	
				690°	704°	732°	735°	740°	745°	750°	755°	760°	765°	770°	775°	780°	785°	816°	871°	927°	982°	1000°
1	Brayshaw	1.16	Shore.....	..	..	..	103	102	101	100	99	99	98	98	96	94	80	..	..	..	..	..
2	Brayshaw	1.16	Brinell....	..	..	..	560	600+	600+	600+	600+	600+	600+	600+	560	600+	495	..	..	..	..	..
3	Brinell....	0.70	Brinell....	241	..	..	..	..	..	744	..	..	..	..	..	..	..	..	..	..	..	744
4	Brinell....	0.66	Brinell....	223	..	..	..	..	..	387	..	..	..	..	..	..	..	..	..	..	..	387
5	Brinell....	0.10	Brinell....	134	..	..	..	..	..	163	..	..	..	..	..	..	..	..	..	..	..	137
6	Shore.....	0.90	Shore.....	..	46	65	..	..	..	..	..	105	..	..	..	..	..	103	101	100	..	..
7	Shore.....	1.00	Shore.....	..	44	50	..	..	..	..	..	103.5	..	..	..	..	104.5	103.5	99.5	92	90	..

NOTE.—The data for specimens Nos. 1 and 2 are collected from the *Proceedings* of the Institute of Mechanical Engineers, May, 1910, p. 571; for specimens Nos. 3, 4 and 5 from the *Journal* of the Iron and Steel Institute, 1901, No. I, p. 293; for specimens Nos. 6 and 7, from "The Shore Sclerescope." Fifth Edition, 1912, p. 44.

temperature, followed by an abrupt loss of hardness as the temperature passes Ar1.

The general features of this progress are like those of Shore for eutectoid steel and of Brinell for steel of 0.66 and 0.70 per cent of carbon, as shown in Table III, a very slight change as the quenching temperature falls towards Ar1 followed by an abrupt loss on passing it. Our slight loss with falling temperature is replaced by constancy of hardness in Brinell's tests and by a very slight gain in the Shore tests.

In hyper-eutectoid steel Brayshaw's results show indeed a considerable increase of hardness as the quenching temperature descends through about this range of temperature, from 785 to 740° C., but here a special cause is at work, the progressive replacement of cementite dissolved in the martensite by free particles of pro-eutectoid cementite, which may well increase the hardness, as indeed may the simultaneous decrease of grain size as the highest temperature reached decreases. The silicon content of his steel was so small that graphitization can hardly have occurred.

10. *Contrast between the Progressive Volume Change and the Constant Hardness of the Quenched Steel.*—This contrast explains itself as representing the complexity of the transformation during rapid cooling, from the initially relatively soft austenite, through the intermediate extremely hard martensite, towards the final pearlite, the softest of all. In so far as restraining the transformation prevents austenite from changing into martensite it makes for softness; in so far as it prevents martensite from changing into pearlite it makes for hardness. Because the volume relation of these three substances need not coincide with their hardness relation, the volume changes caused by variations in quenching temperature need not correspond to the hardness changes. Holding as we do that to lower the quenching temperature is to enable the transformation to go farther, the constancy of the hardness suggests that the hardening effect of the increase in the replacement of austenite by martensite is approximately balanced by the softening effect of the simultaneous replacement of martensite by pearlite. Beyond this, variations in quenching stresses and in grain size may have their effect.

11. *The Effect of Lowering the Quenching Temperature on the Microstructure.*—This effect is shown by the micrographs of Pl. I in conjunction with Fig. *F* of Pl. II. The last shows us the characteristic strong needles, with an "emulsion"[1] ground mass, resulting from quenching from 1100° C. This emulsion has, in many fields of each of the specimens in which it occurs, a strong tendency to form a sort of "plaited" pattern, like that of the strands of common woven basket-ware, as in Fig. *C*, Pl. I, or even a lamellation as in Fig. *H* of Pl. II, much like that of ill-formed lamellar pearlite but often straighter and more continuous (Fig. *H*), or something suggesting strongly the "beaded pearlite" of Benedicks. But in most fields the structure is rather that of an emulsion, as in Fig. *A*, Pl. I, though with less truly spherical masses than those of our familiar emulsions, such as milk, and thus an emulsion with a tendency towards a brecciated[2] or porphyritic[3] structure.

With the low quenching temperature, 797° C. of Fig. *A*, needles are not to be expected, but instead the whole consists of this emulsion. A closely similar structure occurs after quenching from 750°. On quenching from 724° there are some prominent white spots the nature of which is not known. These occur in the outer and harder part of the specimen, while the inner and softer part is free from them, and resembles the specimens quenched from 750° and 797°.

The specimen quenched from 700° consists of very irregular pearlite, in which are some small islands of coalesced cementite. After quenching from 670° and after furnace cooling this same structure occurs, with a slightly greater assembling of the cementite into islands. Indeed the 700° and 670° quenched specimens and the furnace-cooled one might easily be mistaken for each other. True acicular martensite, such as that of Fig. *F*, Pl. II, is induced by quenching from a temperature which for most purposes is improperly high, injuring the quality of the metal both before and after

[1] "*Emulsion.*—A mixture of liquids insoluble in one another, where one is suspended in the other in the form of minute globules, as the fat (butter) in milk."

[2] "*Breccia.*—"A conglomerate in which the fragments, instead of being rounded or water worn, are angular."

[3] "*Porphyritic.*—Composed of a compact homogeneous rock in which distinct crystals or grains of feldspar or some other minerals are embedded." *The Century Dictionary*, 1899.

tempering and increasing needlessly the tendency to crack in hardening.

Without offering a permanent nomenclature for non-acicular martensite, such as that of Figs. *A* and *H*, we call it provisionally "emulsion martensite," conceding that the lamellar regions, such as are there shown, may deserve a special name such as "lamellar martensite." The length and straightness of their apparent threads and strands indicate that they are sections of lamellæ.

Far apart as the genetic conditions of true lamellar pearlite and those of the lamellar parts of emulsion martensite seem at first, they may perhaps represent a common cause, the hasty precipitation of cementite during cooling. Such cementite tends strongly to form broad and often parallel plates, such as are familiar in spiegeleisen, and under favorable conditions in hyper-eutectoid steel cooled slowly from above the transformation range. It is possible that, in the formation both of lamellar pearlite and of the lamellar parts of martensite, the incipient formation of cementite may be along like parallel planes, the octahedral planes of the mother austenite. This incipient formation then gives rise, in the case of lamellar pearlite, to striking sheets of true pearlitic cementite. A like incipient formation of cementite plates may occur even in quenching, and become sufficiently marked to give rise, on etching, to these lamellæ which we find in emulsion martensite. The lamellæ in our eutectoid steel, quenched and then tempered at only 100° C., can hardly represent pure cementite, but rather sheets somewhat richer in cementite than the intervening mass. Even if we hold that the lighter and darker lamellæ represent different stages in the progress of the martensite-pearlite transformation, that difference in stage may well result from the incipient concentration of cementite along these lighter lamellæ.

12. *Temperatures Below the Transformation Range: Volume Changes.*—The constancy of the density as the quenching temperature falls from 700 to 670° C., and as quenching is replaced by furnace cooling to the room temperature, wads *D*, *E* and *F* (Table I), shows that the transformation from austenite into ferrite and cementite had completed itself when

the furnace cooling reached 700° (wad *D*), for any continuation of this transformation would be reflected by a change of density. This accords exactly with Benedicks'[1] important discovery of the rapidity of the transformation itself. He found that after an only moderately retarded water quenching of a steel of 1 per cent of carbon, the electric resistance and specific volume were exactly those of the annealed state.

It agrees also with the results of Langley and Metcalf[2] and of Oknof[3] (Table I).

Quite like our steel, the Langley-Metcalf eutectoid series shows a moderate progressive loss of volume as the quenching temperature falls towards the recalescence, followed by a sudden great loss on passing the recalescence. The same is true of the Oknof eutectoid series, save that its volume changes above the recalescence are very slight and irregular. This series below the recalescence supports ours in showing constancy of volume within the limits of error, on the probable assumption that the specimen quenched at 700° C. had not completed its recalescence.

Oknof's steels of 0.68, 0.70 and 0.83 per cent of carbon follow roughly this law of the progressive loss of volume as

[1] *Journal*, Iron and Steel Institute, 1908, No. II, pp. 219–221. In his retarded quenching 9.5 seconds were occupied in passing from 700° to 100° C.

[2] Langley, "Tool Steel," p. 9. Also *Transactions*, Am. Assoc. Advancement of Science, Vol. 25, 1876, and "The Treatment of Steel," Miller, Metcalf, and Parkin, p. 37, 1881. A bar of steel of 0.871 per cent of carbon and 0.625 in. in diameter was heated differentially so that one end was scintillating white, while the other end remained cold. While in this condition the bar was quenched in water, fragments about ½ in. long were broken off from different parts of its length, and their density was determined. We give a rough estimate of the temperature, which of course varied materially in each fragment. The temperature here given as 750° C. was clearly above the recalescence, because we are told that this specimen became hardened; yet it could not have been far above, because of the color name, "barely showing any red, or very low red hot." Five other high-carbon steels were treated similarly, but we do not consider them, because they were far enough from the eutectoid composition to be affected materially by the precipitation of the pro-eutectoid ferrite or cementite within the transformation range.

[3] Oknof, Ferrum, 1913, Vol. 11, p. 1. Prisms 2 by 2 by 3 cm. of steel of 0.93 per cent carbon, 0.34 per cent manganese, 0.17 per cent silicon, and probably with between 0.02 and 0.07 per cent phosphorus, and 0.02 to 0.03 per cent sulfur, were heated to 1000° C. in nitrogen, cooled in the furnace to the quenching temperatures shown, held from 10 to 15 minutes, and quenched in cold water. Sixteen other steels were tested in the same way, but they were too far from eutectoid to be relevant here. But in passing we may note that the almost perfect constancy of volume of his steels of 0.08 per cent carbon, and 22.56 per cent nickel and 2.72 per cent chromium, the latter evidently austenitic, in spite of variations of quenching temperature from 1000 to 400° C., tends strongly to show that the variations in density of hardened carbon steel are not due in appreciable degree to retention of stresses due to thermal volume changes as distinguished from transformational ones.

the quenching temperature falls towards the recalescence, and the constancy of volume as it falls farther, though one or two of his individual determinations fall seriously out of line. With his steels of between 0.21 and 0.58 per cent of carbon this law is obscured by the important progressive precipitation of pro-eutectoid ferrite, and perhaps, as he infers, by the beta-alpha transformation at A2, while with his hyper-eutectoid steels it is obscured by the generation and re-absorption of graphite.

A like obscuring of this law occurs in the Langley-Metcalf steel of 0.529 per cent of carbon, but not in their hyper-eutectoid steels, probably because these lacked the important graphitization of Oknof's steels, incited in part by his very high heating (1000° C.) and in part by his rather high silicon content.

13. *Hardness Changes.*—But though there is no change of density in this group, our wads *D*, *E* and *F* of Table I, there is a slight progressive softening, which we refer to progressive coalescence, for this would naturally cause that softening, in accordance with the observation that the resistance to deformation is greater at the grain boundaries than in their interior, and hence that the cohesion, whether measured by the hardness, the tensile strength, or the elastic limit, increases with the fineness of the grains.[1] In the present case the increase of coalescence from wad *D* to wads *E* and *F* is not readily verified microscopically, perhaps because the true grains with which we have to do are sub-microscopic. Because coalescence means a decrease of grain fineness, it ought to cause a loss of hardness, that is, a softening.

14. *Circumstantial Evidence of Coalescence.*—This is given by a closer examination of the Shore hardness determinations of wads *D*, *E* and *F* of Table I. Thus whereas the average loss of Shore hardness from wad *D* to wad *F* is 0.9, or about

[1] One of us has given extended data showing that the hardness of steel increases with the grain fineness, and with the deformation which the metal undergoes (Howe, "The Metallography of Steel and Cast Iron," 1916, pp. 358–366). This increase of the hardening effect of grain-fineness as deformation proceeds he refers to the generation of amorphous metal at the grain boundaries by means of the deformation itself, because of the disregistry between the slip planes of adjoining grains. This hypothesis receives unexpected support from the later remarkable results of Fahrenwald ("Practical Substitutes for Platinum, etc.," *Bulletin*, Amer. Inst. Min. Eng., Jan., 1916, p. 123, and reply on discussion, to appear), which show that the hardness of gold may be about quadrupled by increasing both the grain-fineness and the degree of deformation at grain boundaries caused in briquetting the gold.

4 per cent, or of the same order of magnitude as the corresponding loss of 17 Brinell units or about 10 per cent, we have the striking fact that this loss of Shore hardness is accompanied by a progressive increase in the width of the deviations of reading, from 2 units in wad *D* to 6 units in wad *F*. But this is just the effect which should accompany increasing coalescence, that is, the increase in the size of the individual masses of ferrite and cementite. Were the individual masses so large that a single one of ferrite or cementite could occupy the whole field covered by an impact, then the deviation of observed hardness should approach that between the hardness of pure ferrite

TABLE IV.—THE GREAT WEAKENING AND SLIGHT DUCTILIZING EFFECT OF DIVORCING ANNEALING, BY LONG EXPOSURE TO TEMPERATURES JUST BELOW THE TRANSFORMATION RANGE.

Specimen No.	Heat Treatment.	Tensile Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Elongation in 4 in., per cent.	Reduction of Area, per cent.	Fremont Impact Test, kg-m. per 70 sq. cm.
1	Air-cooled from 900° C. to room temperature	139 420	75 420	7.25	16.29	2.6
2	Furnace-cooled from 900 to 690° C., then air-cooled	108 170	51 050	7.75	16.63	1.0
3	Furnace cooled from 900 to 690° C., held 1½ hr. at 690° C., air cooled	105 560	48 500	10.00	16.94	0.75
4	Furnace cooled from 900 to 690° C., held 24 hr. at 690° C., air cooled	96 305	41 870	10.50	19.20	0.6
	Loss (−) or gain (+)	−43 015	−33 550	+3.25	+2.91	−2.0
	Loss (−) or gain (+), percentage of initial	−30.85	−44.48	+44.8	+17.7	−77.0

NOTE.—Each line represents the average of two bars treated together.

and that of pure cementite. The smaller the masses and hence the greater the number covered by a given impression, the less should be the observed difference in hardness between the various spots tested. This agrees with the theory that the progressive decrease in hardness from wad *D* to wad *F* represents progressive coalescence into larger and larger masses. Moreover, because the Brinell ball covers so much larger a field than the Shore, and hence covers a correspondingly greater number of individual ferrite and cementite masses, the deviations between its observed hardness results should be correspondingly less, as they are.

Confirmation of both the softening and the coalescence caused by holding this same steel at temperatures below Ar1 is given in Table IV. These four pairs of specimens, one pair for each treatment, were heated together to 900° C. Pair No. 1 was then air cooled, while pairs Nos. 2, 3 and 4 were furnace cooled to 690°. Pair No. 2 was then air cooled at once, while pairs Nos. 3 and 4 were held at 690° for 1½ hours and 24 hours respectively and then air cooled. The progressive softening recorded in Table IV was accompanied by visible corresponding coalescence, and loss of tensile strength, of yield point, and of resilience as determined by the impact test. Like prolongations of exposure to 690° caused a like loss of tensile strength and yield point for steel of 1.45 per cent of carbon, and a slighter loss of these properties for steels of 0.03 and 0.21 per cent of carbon.[1]

This coalescence should not cause any marked change in density, for it only substitutes larger for smaller particles of the same two constituents, ferrite and cementite. It might indeed have led to a minute gain of density, by decreasing the total number and extent of the boundaries between the individual grains, for these boundaries are thought, not unreasonably, to contain amorphous metal somewhat lighter than the strictly crystallin metal composing the various grains of each constituent.

15. *A Parallel Case in Which Coalescence Softens Steel Without Affecting Its Other Physical Properties.*—The contrast between the progressive softening from wad *D* to wad *F* with the constancy of their specific volume is parallel to the contrast given in Section 21 between the progressive softening in the advanced stages of tempering and the almost complete constancy of the thermo-electric power. So, too, with the progressive decrease of hardness and elastic limit of steel plates as their thickness increases,[2] which may well be referred, at least in large part, to the slower cooling and hence the greater opportunity for coalescence of the thicker plates.

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[1] "Notes on Divorcing Annealing and Other Features of Structural Coalescence in Iron and Steel," *Proceedings*, Cleveland Institute of Engineers, Session 1913–14, No. 6, p. 218, and Figs. 1 to 6.

[2] See for instance Waterhouse, *Iron Age*, 1916, Vol. 97, p. 896.

PART II. EFFECT OF PROLONGING EXPOSURE TO GIVEN TEMPERING TEMPERATURES.

16. *Influence of Time in Tempering Hardened Steel.*—That the softening of hardened steel caused by reheating to a succession of progressively higher temperatures follows a curve of the family of the Brinell and Shore curves of Fig. 3 has been shown with fair concordance by several investigators. The purpose of our experiments covered by this part of our paper is to find the effect of prolonging the exposure to given tempering temperature on the hardness and on the microstructure.

17. *Experimental Conditions.*—The steel used is the eutectoid steel described in Section 2. Wads of this steel, $\frac{5}{8}$ in. in diameter and $\frac{1}{4}$ in. thick, were heated to 1100° C. as described in Section 4, and were then quenched in cold water. They were then tempered by reheating to the temperatures and for the times shown in Fig. 3 and Table V, when their Brinell and Shore hardnesses were determined, and their microstructure examined, with the results shown in Figs. *F* to *I*, Pl. II.[1]

18. *Experimental Difficulties.*—A high quenching temperature, 1100°, was chosen in order to give a well-marked martensitic structure, the break up of which on tempering could be followed the more confidently. But this advantage was dearly bought, for the high quenching temperature led to abundant hardening cracks, and more especially to tempering cracks, shown to be such by their sides being not black but blue, or, more broadly, of the oxide tint corresponding to the time and temperature of tempering. These cracks falsify the hardness determinations.

The results are probably complicated appreciably by the presence of soft spots, less important than those described in Section 6, for their presence was indicated by the very searching Shore test, though neither the Brinell test nor etching disclosed them.

Thus the greatest deviations recorded for given condi-

[1] In this tempering a thick iron plate was brought in the furnace to the tempering temperature, withdrawn for a moment to receive the specimen, and at once returned. In the few cases in which the temperature of the furnace was changed measurably by this operation it returned in two or three minutes to the point sought.

tions in Table V, amounting to about 8 per cent of the minimum hardness in any one set, give an exaggerated notion of the real deviations between different parts of a given wad, first, because they compare wads which were not quenched simultaneously and hence may have differed in their quenching conditions, and second, because these greatest deviations are chiefly among hardness numbers approaching 600, the limit beyond which the results of this method cease to be trustworthy.[1] The 70-

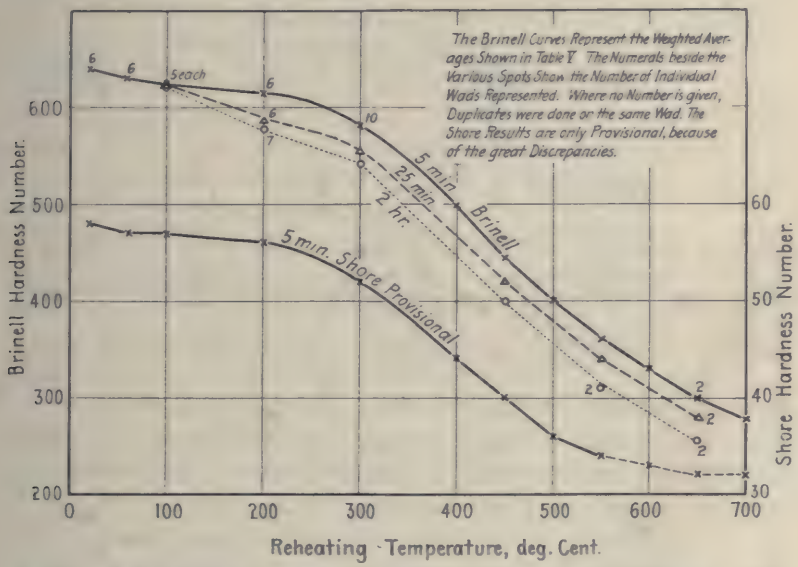


FIG. 3.—The Softening of Hardened Eutectoid Steel with Increasing Time and Temperature.

per-cent deviation of the 724° quenched wad of Table I, on the other hand, represents one single wad, and refers to a range, from 341 to 578, in most of which the method is trustworthy. The greater the hardness, and hence the smaller the permanent impression, the greater is the influence of a given absolute

[1] As the hardness increases the Brinell impressions become both smaller and more irregular, and on both accounts harder to measure accurately. This led Brayshaw (*op. cit.*, p. 557) to question all his results which gave the hardness of hardened steel as greater than 600.

error in measuring the size of that impression. The discordance was greatest in the hardened and untempered wads, and decreased, though not very regularly, as the time and temperature of tempering increased.

TABLE V.—THE SOFTENING OF HARDENED EUTECTOID STEEL WITH INCREASING TIME AND TEMPERATURE OF TEMPERING.

Temperature of Reheating, deg. Cent.	Time of Reheating. 5 min. Brinell Hardness Number. Individual Tests.	5 min. Weighted Average.	25 min. Individual Tests.	25 min. Weighted Average.	2 hr. Individual Tests.	2 hr. Weighted Average.
20	601, 627, 627 653, 653, 653	640	....	....	....	....
60	601, 627, 627 627, 627, 653	630	....	....	....	....
100	601, 627, 627 627, 627	624	601, 627, 627 627, 627	624	601, 601, 627 627, 627	620
200	601, 601, 601 614, 627, 627	616	555, 578, 578 590, 601, 601	587	555, 555, 567 578, 590, 590 601	578
300	555, 555, 578 578, 578, 578 590, 601, 601 601	580	555 (555)	555	534 (545)	541
400	495 (505)	501	....	....	....	....
450	444 (444)	444	415 (422)	420	401, 401	401
500	401 (408)	406	....	....	....	....
550	363 (363)	363	341 (341)	341	302, 311	308
600	331 (336)	334	....	....	....	....
650	293, 302	299	277, 277	277	255, 255	255
700	277, 277	277	....	....	....	....

NOTE.—Each of these determinations was intended to be made at the center of the wad, and each represents a separate wad, except those in parentheses, each of which was made at an uncentral spot on one of the wads represented by one of the other results. In many cases the individual wads for a given time and temperature of quenching were quenched and tempered separately. Note that each number in parentheses is either identical with that of the central test on the same wad, or from 5 to 10 numbers greater, probably because of some hardening caused by the prior and central test, or conceivably to persistence of the greater hardness of the outer ring. The average excess is only 3.2 units, or within the limits of observational error. Because of this probable hardening these numbers in parentheses are recorded primarily for verification of the accompanying central tests.

The cracks were usually invisible to the naked eye before the Brinell test, and were developed by the test itself. Moreover, the wads which had the smallest ball-impressions and thus were reported as the hardest were in general the most

cracked. This result is surprising, because the crack itself should lower the elastic limit of the wad taken as a whole, and thus lead to greater yielding and hence to a wider Brinell impression and less observed hardness. It suggests that the very hardest, which were also naturally the most brittle, were so much harder than the next in hardness that they retained some of their excess of observed hardness in spite of the weakening effects of the cracking, which, for that matter, often occurred during the Brinell test itself.

19. *Weighting the Brinell Averages.*—Because of this cause of error, the Brinell determinations were repeated, the number of repetitions increasing with the deviation between results, being for instance specially great for the hardened but untempered state, and for the low-temperature temperings. The number of wads represented by each Brinell spot in Fig. 3 is shown by the numeral beside it.

Because the serious probable causes of error, such as these cracks, bad quenching, and surface decarburization, all tend to lessen the observed hardness, a weight of 2 was assigned to the highest results for each state, of $1\frac{1}{2}$ to the intermediate ones, and of 1 to the lowest ones.

20. *The Shore Hardness.*—It was not till after this investigation had progressed far that we decided to apply the Shore test to the wads already hardened and tempered. Our Shore results are so very erratic that we mark the Shore curve of Fig. 3 "provisional" and that we give the results for the 5-minute temperings only, suppressing those for 25 and 60 minutes. We have rejected many results arbitrarily. The results are in general concordant for each face of a given wad, whereas different wads may vary by 7 units or more.

We incline to refer the discrepancy to the thinness of our wads, only about $\frac{1}{4}$ in., first because our results with thicker wads of this same steel, when annealed and when quenched from below the recalescence, gave concordant Shore results (Sections 7 and 13), and second, because, among the tempered wads of this present series, the deviations increase in general with the tempering, whereas this increase in tempering ought to lessen progressively the most important probable source of deviation, variations in the quenching conditions.

Thus 30 determinations on 7 wads hardened and tempered at 60° for 5 minutes varied only from 53 to 58 in hardness, or by 9.4 per cent of the minimum hardness, whereas 15 determinations on 2 wads hardened and tempered at 650° for 2 hours varied from 19 to 32 in hardness, or by 75.8 per cent of the minimum hardness.

But if the deviations are due to the thinness of the wad, that is to its flexibility, then they should increase with the tempering, because this lessens the elastic limit, and thus increases the flexibility.

Against this explanation has to be set the uniformity of

TABLE VI.—TO PROLONG THE TEMPERING TIME DECREASES THE HARDNESS.

Brinell Hardness Number	Temperature needed to lessen the hardness to the number given in Column A, deg. Cent.		Difference, Column B minus Column C.
	In 5 min.	In 2 hr.	
A	B	C	D
600	260	150	110
500	400	345	55
400	500	450	50
300	650	575	75
280	695	605	90

our Shore tests of our $\frac{1}{16}$-in. silver plate, Table II, with a total range of 1 unit, or 18 per cent of the minimum.

20*a*. *The Results.*—Both the Brinell and the Shore results agree in showing first that the curve is much flatter at the extreme left than farther to the right, and second that even at relatively high temperatures the influence of time is very marked. This is shown further in Table VI. For instance, a 2-hour exposure to 605° C. reduces the hardness as much as a 5-minute exposure to 695°. The last column, indicating how much higher the temperature must be to give in a 5-minute exposure the softening effect that is given in 2 hours at given lower temperature, must be taken as a very rough approximation, because of the flatness of these curves.

21. *The Time Effect Probably Represents Coalescence.*—Barus and Strouhal[1] showed that the time effect of tempering on the thermo-electric power of hardened steel, though it was very great at low temperatures, 66° and 100°, yet was extremely small at 330°, and apparently immeasurably small at 1000°. For instance, prolonging the exposure to 330° from 1 minute to 1½ hours increased the effect on the thermo-electric power by only 2.5 per cent. It is reasonable to assume that any continuation of the chemical transformation from the hardened or martensite to the annealed or ferrite plus cementite state, would be reflected by a corresponding continuation of the resultant change in the thermo-electric power, and hence that this chemical change reaches a state very near to completion very rapidly on reheating to 330°.

Hence we infer that the chemical transformation was almost complete in 1 minute at 330°, which agrees with the general observation that the transformation proper is very nearly or quite complete on reheating hardened steel to 400°.[2]

Radically different is the law governing the actual softening of hardened steel, that is, the loss of its Brinell and Shore hardness. For whereas the influence of tempering-time on the chemical transformation, as shown by Barus and Strouhal, is great at low temperatures and becomes less as the temperature rises, being insignificant at 330°, its influence on the softening seems at least as great at 600° and 650° as at 100° and 200°.

That the influence of time on the tempering effect of exposures to 440° is very great is shown by Mathews and Stagg.[3] Their hardened steel of 0.50 per cent carbon, after it had been held at 427° for 8 minutes, lost 35 additional Brinell units on prolonging the exposure to 20 minutes, and 50 more units on

[1] "Physical Characteristics of the Iron Carburets." *Bulletin, No. 14*, U. S. Geological Survey, 1885, pp. 54–56. "The annealing effect of any temperature increases gradually at a rate diminishing continuously through infinite time, very slowly in case of low temperature (less than 100°), with extreme rapidity in case of high temperature (greater than 200°)."

[2] See, for instance, Report on the Nomenclature of the Microscopic Substances, *Proceedings*, Internat. Assoc. Test. Mats., VI Congress, 1912, Vol. 1, II 7, p. 17. Sorbite is the stage of the transformation brought about "by reheating hardened steel to a little above 400°." "Most writers believe that it is essentially an uncoagulated conglomerate of irresoluble pearlite with ferrite in hypo- and cementite in hyper-eutectoid steels respectively, but that it often contains some incompletely transformed matter."

[3] *Transactions*, Am. Soc. Mech. Eng., Dec. 8, 1914, table on p. 20 of reprint. Cp. *Iron Age*, Vol. 94, p. 1340 (1914).

again prolonging it to 40 minutes, so that the loss per minute in this second prolongation, 2.5 units, was nearly as great as that in the first prolongation, 3 units.

Again this second prolongation of the tempering at 440° caused a loss in elastic limit of 26,125 lb. per sq. in., or almost as much as the loss during the first prolongation, 27,625 lb. per sq. in.

Because these prolongations of the time of tempering at these relatively high temperatures cannot cause any important continuation of the chemical transformation, we refer the softening which they cause to the coalescence of the sub-microscopic particles of ferrite and cementite resulting from the completion of the transformation, as already pointed out in Section 13, for this coalescence, though it should affect neither the density nor the thermo-electric power, is wholly competent to cause just this softening.

22. *Microstructural Changes in the Progressive Tempering of Hardened Steel.*—The quenched and untempered specimens contain when etched many bright martensite needles, varying from white to light blue and light brown. They are contained in a dark brown mass which itself is in large part acicular, and in part a conglomerate of lighter and darker substances, here brecciated or porphyritic, there with rounded masses. The darker often form irregular threads, the light often suggest similarity to the martensite needles themselves. Most of these needles are crossed by dark threads of which many are parallel to the length of the needle, though many others cross it, and sometimes irregularly. Some of the needles are very compact and almost free from black threads. The structure is that shown in Fig. *F*, Pl. II, for the 5-minute tempering at 60° of this specimen did not affect its structure materially.

The contrast between the needles and the ground mass reminds one of that between primary cementite and the eutectic, but it is not so strong.

The proportion of needles to ground mass varies very greatly from field to field.

After a little practice one recognizes in places a general parallelism or stranding of the black threads in the ground

mass. The regularity and prominence of this stranding vary very greatly in different fields.

Two or perhaps three distinct features of the microstructual change with rising tempering temperature may be noted. First, very early in the tempering the initial martensite needles thin out and disappear, and the place which they had occupied becomes filled with a ground mass apparently identical in structure with the initial, as in Fig. *H*, Pl. II. Second, the plaiting, layering, or parallelism found in the quenched and in the slightly tempered specimen gradually fades out. Third, the particles coalesce slowly into coarser masses.

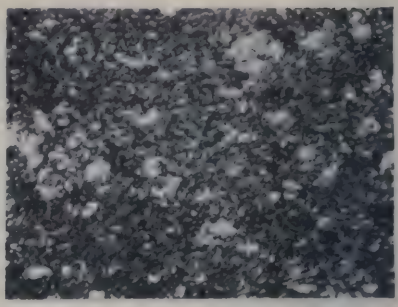
Of these changes the disappearance of the initial thick white needles is by far the most striking, yet it is accompanied by very little softening. Thus, though a 5-minute exposure to 60° hardly affects the microstructure, so that the structure of Fig. *F*, Pl. II, which represents this treatment, is hardly to be distinguished from that of the hardened and untempered steel, a 5-minute exposure to a temperature only 40° higher, 100°, thins these needles greatly, as shown in Fig. *G*. Yet the softening even in a 2-hour heating to 100°, as shown in Table V, is so slight as hardly to exceed the limits of experimental error, if we admit with Brayshaw that the Brinell method is hardly trustworthy for hardness in excess of 600.

A 5-minute exposure to 300° leaves only very small fragments of the original white needles, yet it has little effect on the general parallelism or stranding over considerable areas. There are still many areas with as good parallelism as that of Fig. *H*, and after 5 minutes at 400° such areas are still to be found, yet after 5 minutes at 450° there are few traces of parallelism.

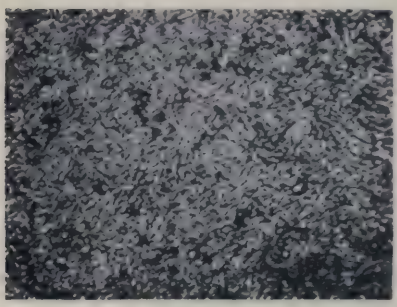
This parallel structure thus exists in troostite, for such high carbon steel when hardened and reheated thus to between 300° and 400° is troostite by definition and changes to sorbite on heating beyond 400°, that is, on breaking up this parallelism. Thus the structure of this troostite is closely like that of the ground mass between the martensite needles of the quenched and untempered steel.

The third feature of the structural change, the coalescence of the cementite, is very difficult to detect, at least during the

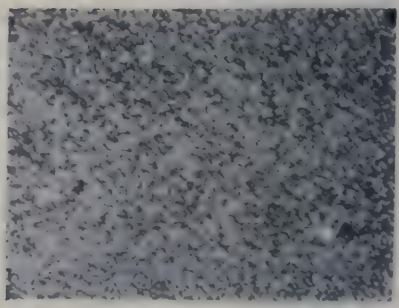
A.—Quenched at 797° C. (×500).



B.—Quenched at 724° C.; Outer Part (×500).



C.—Quenched at 724° C.; Central Part (×500).

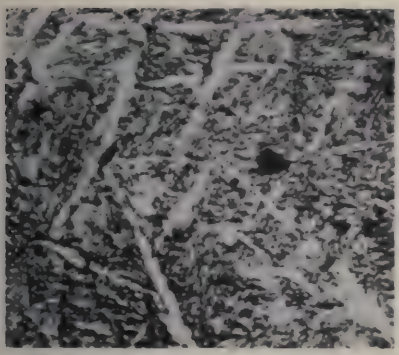


D.—Furnace Cooled (×500).

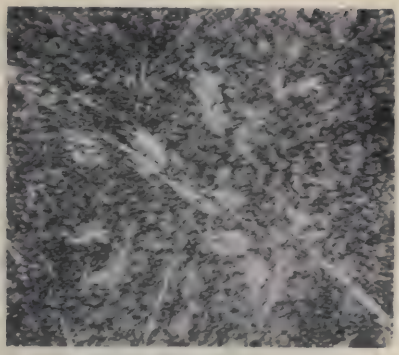
E.—Quenched at 724° C. (×2.3).

All specimens of 0.92-per-cent carbon steel, heated to 797° C., cooled to the temperature indicated and quenched.

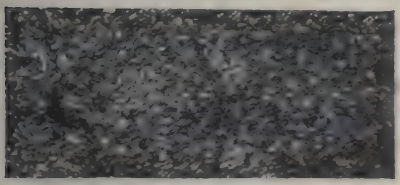
PLATE I.—VARIATIONS IN THE MICROSTRUCTURE WITH THE QUENCHING TEMPERATURE.



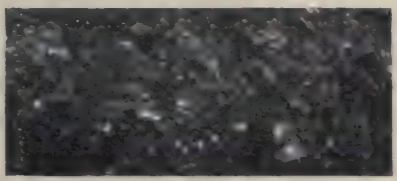
F.—5 Minutes at 60° C.
Conglomerate and Thick
Needles (×500).



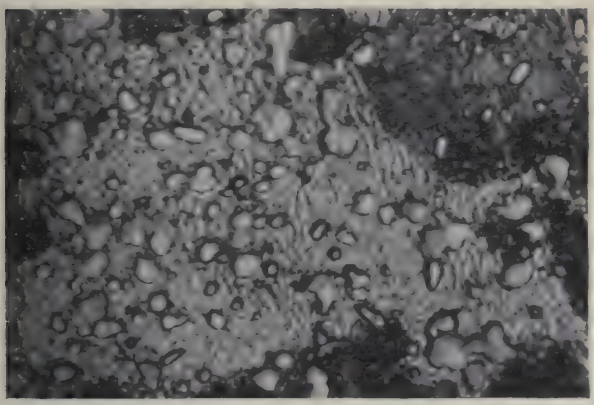
G.—5 Minutes at 100° C.
Conglomerate and Thin
Needles (×500).



H.—2 Hours at 100° C.
Conglomerate (×500).



I.—5 Minutes at 450° C.
Emulsion with Traces of Coalescence
(×1200).



J.—Quenched, then Annealed at about 600° C.
Granular Pearlite (×1200).

Specimens *F*, *G*, *H* and *I*, steel of 0.92 per cent carbon, quenched from 1100° C., and reheated as shown.

Specimen *J*, steel of 1.1± per cent carbon, treated by W. Campbell.

PLATE II.—INFLUENCE OF TEMPERING TEMPERATURE ON THE MICROSTRUCTURE.

early stages covered by our experiments, so that, without much practice, one might well fail to distinguish for instance a specimen heated for 5 minutes to 450° from one heated to 700°.[1] Hence Fig. *I*, Pl. II, should be taken rather as illustrating a stage between those of Fig. *H* and of Fig. *J*, than as typical of a 5-minute 450° tempering. In both the 5-minute 450° and the 5-minute 700° specimens there are on hand many areas much like Fig. *H*, and on the other hand many like Fig. *I*.

Thus, however convenient the microscope may be for distinguishing between the early stages of tempering, we question its convenience for detecting the later ones.

Though the general progress of this coalescence is towards the advanced stage shown in Fig. *J* of Pl. II, we do not find any fields even after the longest and highest tempering, in which it has gone so far that the structure is rather that of "granular pearlite" than that of "sorbite."

Because the change in microstructure is so gradual and slight, and because the different fields in a given specimen differ so greatly in structure, we have failed to trace here with confidence the influence of tempering time as distinguished from tempering temperature.

23. *Fracture.*[2]—The shock fractures of a series of specimens of the eutectoid steel, quenched from 1100°, and tempered at the same temperatures and for the same times as the wads of Fig. 3 and Table V, were examined.

The fracture of the hardened specimens is bright and rather coarsely crystallin, with many shining fiery facets. No marked change in the fracture occurs till the tempering temperature reaches 400°, when the fracture is appreciably darker, so that one picks out a 400° tempered piece rather readily from among the untempered ones. As the tempering temperature rises higher, the fracture becomes still darker and finer, reaching at 550° the full darkness, a very dark gray, and complete earthiness, and changing relatively little with further rise, except that it becomes gradually smoother and freer from little roughnesses.

[1] This agrees in general with the observations of McWilliams and Barnes, *Journal*, Iron and Steel Institute, 1909, No. 1, p. 369.

[2] The fractures were made in testing the hardened and tempered pieces with the Fremont impact test for notched bars.

As the tempering temperature passes 450° the change in fracture is very rapid. Thus the change from the 5-minute 450° to the 2-hour 450° specimen is striking.

The influence of time is marked. Thus the fracture of the 2-hour 450° specimen is farther advanced, that is, darker and duller, than that of the 5-minute 500° one.

The fracture of the well-tempered specimens has the peculiar look called "chatoyance," which may perhaps be best described as that of watered silk. That is to say, as the frac-

TABLE VII.—INFLUENCE OF TIME AND TEMPERATURE ON THE OXIDE COLORS.

Tempering Temperature, deg. Cent.	Colors in Tempering Cracks.			Other Observers;[1] Colors on Outer Surface.
	Tempered for 5 min.	Tempered for 25 min.	Tempered for 2 hr.	
200	.....	.....	Yellow	Straw
230	.....	.....	.....	Straw
243 - 250	.....	.....	.....	Yellow
255 - 275	.....	.....	.....	Brown
277	.....	.....	.....	Purple
300	Light brown	Blue	Purple	Violet
325	.....	.....	.....	Violet
288 - 350	.....	.....	.....	Blue
316 - 371	.....	.....	.....	Dark blue
400	Dark blue	.....	.....	Blue gray
450	Very dark blue	Very dark	.....	Gray

[1] Howe, "Metallography of Steel and Cast Iron," McGraw-Hill Book Co., 1916, p. 621.

ture is turned from one position to another, certain definite areas shine out momentarily more brightly than the rest, each area being of nearly uniform brightness. Each of these areas is itself rough, showing under a lens many irregular surfaces, just as one of the bright areas in watered silk would.

For temperatures from 400° to 550° the indications of the fracture are much clearer than those of the microstructure.

23*a*. *Oxide Colors.*—The oxide colors found in the cracks of the quenched-and-tempered Fremont test pieces show the

influence of time as well as of temperature. Thus of the 300° specimens the 5-minute one was light brown, the 25-minute one blue, and the 2-hour one purple. The colors may be recapitulated as in Table VII.

Because of the poor circulation of air in these tempering cracks the oxide tint, for each of our periods of time, should be somewhat lighter than that reported by other observers who report the color of the outside of the specimen.

PART III. NOTES ON THE SHORE SCLEROSCOPE TEST.

24. *General Considerations.*—The Shore hardness is measured by the height of the rebound of a weight dropped on the surface of the specimen, which it indents permanently more or less, a height which represents the striking energy of the weight minus such dissipating quantities as the atmospheric friction, the local heating, and more especially the work done in deforming the specimen permanently. Clearly the permanent displacement of the particles which are struck directly is resisted not only by their own elasticity, but also by that of any other particles which have to be displaced in order to permit that displacement, so that the Shore hardness ought to be less for a very thin specimen than for a thicker one, and less at the very edge or corner of a piece than in its middle, because not only the metal in the line of the impact but also that in planes at right angles to that line must be displaced in order to permit the permanent indentation made by the falling weight. We now illustrate first the effect of thickness, or vertical support of the place struck, and then the effect of lateral support.

25. *The Observed Hardness Increases with the Thickness of the Specimen.*—The numbers in columns F and K of Table VIII, representing the average observed hardness of the hard martensitic ring and of the soft central area respectively, of hardened and untempered wads of our eutectoid steel, all hardened under like conditions, and also of hardened and tempered wads, show a progressive increase of observed Shore hardness as the thickness of the wads increases, though the true hardness of the hardened wads probably varies in the opposite direction, for the slower cooling of the thicker wads should lead to less thorough hardening. The increase of the observed

hardness with the thickness of the wads evidently represents the greater rigidity of the specimen regarded as an anvil whence the rebound of the ball occurs.

Within what limits this law holds good we do not know, but we believe that for such narrow objects as ours it must be true of thicknesses much beyond the 0.625 in. of our thickest

TABLE VIII.—INCREASE OF THE SHORE HARDNESS WITH THE THICKNESS OF THE SPECIMEN.

SPECIMENS HELD 5 MIN. AT 1100° C., THEN QUENCHED IN COLD WATER.

Specimen No.	Thickness, in.	Shore Hardness Number.										Excess of Hard, Average, Column F minus Column K.	Excess of Minimum Hard over Maximum Soft, Column E minus Column I.
		Hard Ring.					Soft Core.						
		Number of Tests.	Maximum.	Minimum.	Average.	Range.	Number of Tests.	Maximum.	Minimum.	Average.	Range.		
A	B	C	D	E	F	G	H	I	J	K	L	M	N
2	0.141	4	56	56	56	0	3	49	46	47.7	3	8.3	7
4	0.142	6	58	56	56.5	2	4	54	52	53	2	3.5	2
11	0.161	5	56	55	55.8	1	5	47	48	47.5	1	8.3	8
5	0.175	5	57	54	56	3	4	51	46	49	5	7	3
3	0.176	7	57	55	55.9	1	3	46	45	45.7	1	10.2	9
12	0.260	5	58	57	57.8	1	4	48	47	47.5	1	10.3	10
13	0.377	6	59	57	58.2	2	3	48	48	48	0	10.2	9
14	0.491	8	66	61	63.9	5	5	54	52	52.6	2	11.3	7
15	0.620	6	67	65	66	2	7	56	54	55.7	2	10.3	9

SPECIMENS HELD 5 MIN. AT 1100° C., QUENCHED IN COLD WATER, THEN ANNEALED FOR 5 MIN. AT 700° C.

		Inner Parts.					Circumferential Parts, Ill Supported.						
87	0.25	7	32	32	32	0	6	26	20	23.7	6	8.3	5
85	0.25	8	32	32	32	0	6	26	23	24.8	3	7.2	6
87	0.62	14	35	35	35	0	9	28	25	25.6	3	9.4	7
88	0.62	24	35	32	34.3	3	14	32	25	27.6	7	6.7	0

piece, because the increase shows no signs of dying out as this thickness is approached. Part of the irregularity of the increase of hardness with increasing thickness is referable to variable degrees of quenching-cracking. Thus the relative softness of the 0.377-in. wad is referable to its extreme cracking.

The hardness of the soft core increases with increasing

thickness much less regularly than that of the hard ring, yet in spite of this irregularity the excess of hardness of the hard ring over the soft core varies little from end to end of the series, being about 9 Shore units and perhaps increasing slightly with the thickness of the specimen.

In the annealed state, that is, after heating the hardened specimens to 700° for 5 minutes and furnace cooling wads Nos. 87 and 88, the effect of thickness is much less than in the hardened state, an increase of thickness from $\frac{1}{4}$ to $\frac{5}{8}$ in. increasing the observed hardness by only 3 units against an increase of about 9 units for the hardened state.

26. *The Harter Investigation.*[1]—Fig. 4 and Table IX illustrate the increase of the observed hardness with the thickness. Thus the lateral support of the metal struck on faces *B* and *D* is only two-thirds of that on the median line of spots *b*, *e*, *h*, *k*, of the broad faces *A* and *C*, $\frac{1}{8}$ in. instead of $\frac{3}{16}$ in. But this deficit of lateral support on edges *B* and *D* is outweighed by their greater thickness in the line of impact, $\frac{3}{8}$ in. against $\frac{1}{4}$ in., and the average hardness on the edges is about 12 per cent greater than at the line *b*, *e*, *h*, *k*, taking this latter as 100. This shows that, with metal of this hardness, a thickness of $\frac{1}{4}$ in. is not enough to ensure true hardness readings.

27. *The Observed Hardness Increases with the Distance from the Edge of the Specimen.*—This is shown by the data of Fig. 4 as summed up in Table IX. Here, confining our attention to the broad faces *A* and *C*, the average hardness is greatest at the four interior spots, next at the outer spots other than the

[1] These results were communicated to one of us on July 9, 1914, by Mr. Isaac Harter, Jr., Superintendent of the Babcock & Wilcox Co., whom we thank heartily for permission to publish them. More than 2496 Shore determinations were made on 13 strips, each 2 in. by $\frac{1}{2}$ in. by $\frac{1}{4}$ in., cut from beside each other from a plate of steel of 60,000 lb. per sq. in. tensile strength or say of 0.20 per cent of carbon. These strips were heated to 927° C. (1700° F.) "for a short time" and then quenched in oil at 149° C. (300° F.). Each strip was then tested twice at each of 32 spots. Each of the two sketches in Fig. 4 shows the arrangement of these spots on two of the main faces of each strip. Each of the two tests consisted of at least three individual readings taken near each other, but of course not at exactly the same place. The second test at each spot was made after filing away all traces of the marks made by the first test. The average of the second set of readings is slightly greater than that of the first at 24 out of the 32 spots, showing that the hardening caused by the first set of tests extended beyond the region removed in filing away the traces of their impressions. The hardness of these specimens, 13.84 on an average of the 2496 results, and in many cases only 3, though it is indeed surprisingly far below the normal hardness of such steel, about 26, is yet no doubt true, and represents an abnormal state of the metal brought about by certain special conditions then under study.

TABLE IX.—THE OBSERVED SHORE HARDNESS INCREASES WITH THE THICKNESS OF THE METAL IN THE DIRECTION OF IMPACT, AND WITH THE DISTANCE FROM THE OUTER EDGES. HARTER'S DATA.

Position and Designation of Points Taken.	Shore Hardness Number.
BROAD FACES, *A* AND *C*.	
Central; *e*, *h*	18.04
Edges except corners; *d*, *g*, *i*, *f*	13.04
Corners; *a*, *j*, *l*, *c*	10.36
Median line; *b*, *e*, *h*, *k*	15.08
NARROW FACES, *B* AND *D*.	
Central; *n*, *o*	17.64
Ends; *m*, *p*	16.07
Average of edges; *m*, *n*, *o*, *p*	16.86

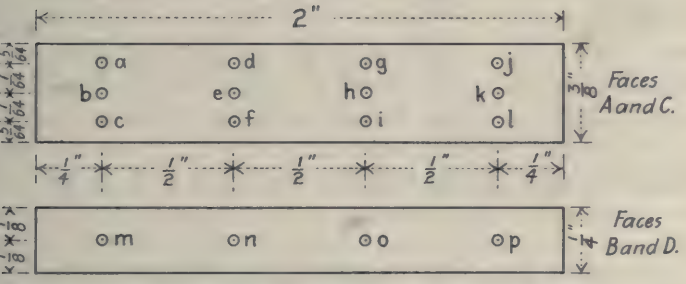


FIG. 4.—Arrangement on the Faces of the Harter Strips of the Spots at which the Shore Hardness was Determined.

corner ones, and least at the corners.[1] Thus with metal of this hardness and thickness, readings taken within $\frac{5}{64}$ in. (0.078 in.) of the edge are too low.

This conclusion refers only to the absolute hardness number. Whether trustworthy relative readings with specimens all of the same thickness can be taken at spots only $\frac{5}{64}$ in. (0.078 in.) from an edge we do not know.

TABLE X.—THE OBSERVED SHORE HARDNESS IS INCREASED BY SUPERPOSING WELL-FITTING SPECIMENS.

ALL WADS QUENCHED IN COLD WATER AFTER STAYING 5 MIN. AT 1100° C.

Line No.	Wad No.	Thickness, in.	Shore Hardness Number. Hard Ring. Number of Tests.	Maximum.	Minimum.	Average.	Range.	Soft Core. Number of Tests.	Maximum.	Minimum.	Average.	Range.	Excess of Hard over Soft, Column F minus Column K.
	A	B	C	D	E	F	G	H	I	J	K	L	M
1	11	0.161	5	56	55	55.8	1	4	47	46	46.8	1	9
2	11 on 15	0.781	1	..	..	58	..	6	48	46	47	2	11
3	Gain (+) or loss (−)		..	..	..	+2.2	..	..	..	..	+0.2	..	....
4	11 on 15, reground		6	66	63	63.8	3	5	56	56	56	0	7.8
5	Gain (+) or loss (−) over 11 alone.		..	..	..	+8.	..	..	..	..	+9.2	..	....
6	11 on 14	0.652	7	66	66	66	0	6	58	56	57	2	9
7	Gain (+) or loss (−)		..	..	..	+10.2	..	..	..	..	+10.2	..	....
8	12	0.260	5	58	57	57.4	1	4	48	47	47.5	1	9.9
9	12 on 14	0.751	5	65	64	64.4	1	5	56	56	56	0	8.4
10	Gain (+) or loss (−)		..	..	..	+7	..	..	..	..	+8.5	..	....
11	3	0.176	8	57	55	55.9	2	3	46	45	45.7	1	10.2
12	3 on 14	0.667	7	66	64	65.1	2	4	56	55	55.8	1	9.3
13	Gain (+) or loss (−)		..	..	..	+9.2	..	..	..	..	+10.1	..	....
14	3 on 2, 5 and 6	0.643	7	57	55	55.6	2	3	46	45	45.7	1	9.9
15	Gain (+) or loss (−) over 3 alone		..	..	..	−0.3	..	..	..	..	0	..	....
16	3 on 1, 2, 4, 5, 6, 7 and 8	1.277	9	46	40	44.2	6	5	35	33	34	2	10.2
17	Gain (+) or loss (−) over 3 alone		..	..	..	−11.7	..	..	..	..	−11.7	..	....

In softer and thinner metal the distance from the edge needed to give constant results is less. For instance, with sheet silver $\frac{1}{16}$ in. thick, the hardness numbers which we observed at $\frac{1}{16}$ in. from the edge were not appreciably less than those

[1] This is true of an average of the eight corners, though at two of them the hardness was greater than at six of the better supported points. Because of local variations in conditions, representing the abnormal exposure already referred to, the deviations in these data are so great that only the teachings of the averages of large numbers of observations deserve weight.

found farther in. Probably the vertical yielding in such thin and soft sheets is so easy that little radial pressure arises.

28. *The Observed Shore Hardness is Increased by Superposing Well-Fitting Specimens.*—Table X illustrates this law. Several of the wads of our eutectoid steel, quenched from 1100° and not tempered, were tested both alone and when set one on another. Note that this superposition on a single wad always increases the observed hardness, by 2.2 units in line No. 3, 10.2 in line No. 7, 7 in line No. 10, and 9.2 in line No. 13 for the hard ring, and by roughly corresponding amounts for the soft core. Moreover, increasing the accuracy of the fit by re-grinding the wads increased the gain from 2.2 to 8 and from 0.2 to 9.2 units in the hard and soft parts respectively, in line No. 5.

But when a single wad is superposed on several, the cumulative softening effect of the several inaccuracies of fit outweighs the stiffening effect of the increase of mass, with the result that the superposition may fail to increase and may even lessen the observed hardness. Thus to superpose wad No. 3 on three others failed to increase its observed hardness (line No. 15), and to set it on seven others lowered the observed hardness by 11.7 units, both in the hard ring and the soft core (line No. 17).

29. *Increase of the Hardness with Repetitions of the Test in the Same Spot.*—Because the Shore test, like the Brinell, passes the elastic limit of the substance tested, and hence causes an appreciable degree of plastic deformation, a second test applied in the same spot should find the metal already hardened by the first test, and hence should give a greater hardness number. And this actually occurs, the hardening increasing progressively up to a certain point, as is shown by the results plotted in Figs. 5, 6 and 7, representing the variations in the observed Shore hardness found by repeated tests at a given spot, without unclamping the specimen, or moving it or the scleroscope in any way.

The results shown by the unbroken line (—×—·×—) in Fig. 5 represent a specimen of the American Rolling Mill Company's ultra low-carbon steel (A. R. M. steel), which is almost pure ferrite. This was clamped in the usual way, and

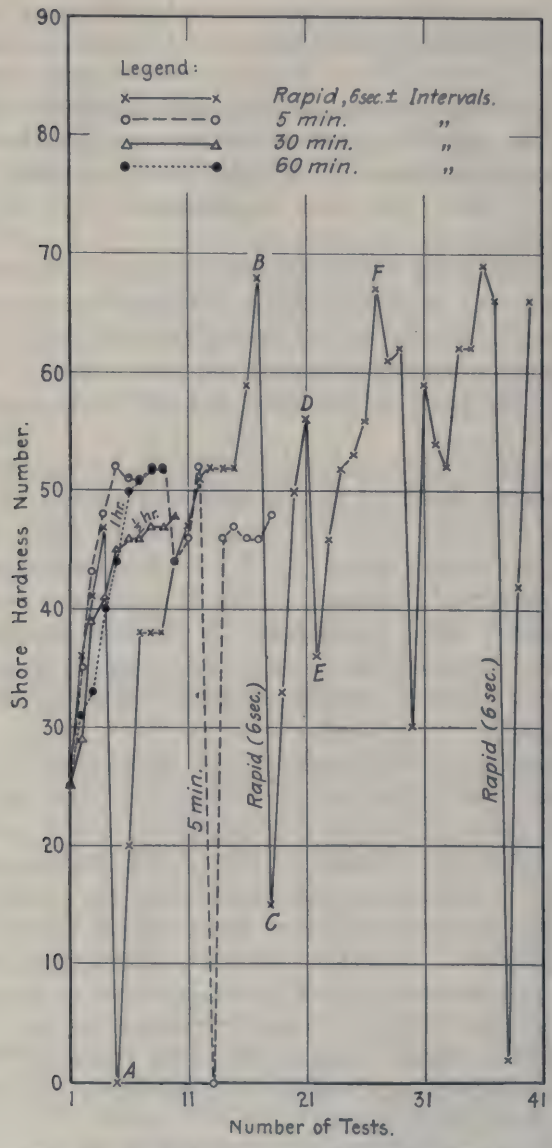


FIG. 5.—Irregular Increase of the Shore Hardness of Ferrite (A. R. M. Steel) as found by Repeated Tests at a Single Spot.

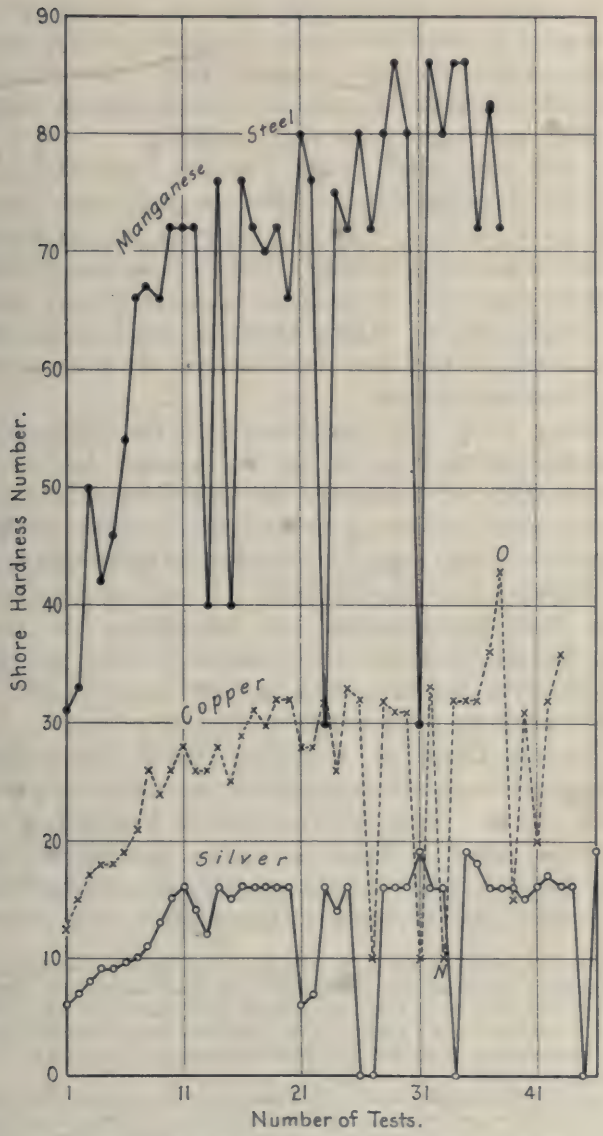


FIG. 6.—Irregular Increase of the Shore Hardness of Manganese Steel, Copper and Silver as found by Repeated Tests at a Single Spot.

a series of hardness readings was taken at one and the same spot as rapidly as possible, actually at about 6-second intervals. The corresponding readings shown in Fig. 6 were then made for Hadfield's manganese steel or manganiferous austenite, for Lake Superior copper, and for pure silver.

The other data shown in Fig. 5 were obtained with the A. R. M. steel by making like repeated hardness tests, each series at a single spot, one series at 5-minute intervals, one at 30-minute, and one at 60-minute intervals.[1] The "1 hour and additional" curve represents a set of readings taken at 1-hour intervals, further complicated by taking after the second-hour interval and after each of the later ones several additional readings at about 6-second intervals.

Awaiting fuller data, we have here the suggestion that the hardness increases up to the seventeenth test of ferrite, the twenty-ninth of austenite, the thirty-seventh of copper, and the thirty-first of silver. Great variations are to be expected in the number of tests needed to develop the maximum hardness.

If, allowing for the difficulty of reading results which differ far from the preceding and hence from the expected, we omit the tips of some of the sharpest peaks, we still find that the observed hardness of all four of the metals thus tested is nearly tripled[2] by this repetition of tests.

30. *Sudden Drops in Hardness.*—While this progressive hardening would naturally be expected, the sudden drops in hardness might not be. Thus with each of the four metals studied, the hardness, after a nearly or quite continuous increase, decreases perfectly abruptly and greatly, even to zero in some tests of ferrite and of silver. These zeros, and indeed the

[1] The A. R. M. steel was a block $2\frac{1}{2}\frac{5}{8}$ in. by $1\frac{1}{1}\frac{1}{6}$ in. by 1 in., stamped as containing 0.026 per cent of carbon. The manganese steel specimen had been quenched from 1100° in cold water. It was 10 in. by $\frac{3}{4}$ in. by $\frac{1}{4}$ in. The intervals were about $4\frac{1}{2}$ seconds. The copper was a wad $\frac{3}{4}$ in. thick cut from a rolled bar 1 in. in diameter, and the intervals were about 6 seconds. The silver was "Assay sheet," annealed, $\frac{1}{16}$ in. thick by $\frac{1}{2}$ in. by $\frac{3}{4}$ in. The intervals were about $4\frac{1}{2}$ seconds.

[2] The power to give so great a degree of hardening here without rupture, as in wire drawing, is referable to the support which the metal has on all sides. Goerens, for instance, found that the Shore hardness of steel of 0.27 per cent of carbon was raised to 265 per cent of the initial hot rolled state by 7 wire drafts with a total of 74.2 per cent reduction. (*Carnegie Memoirs*, Iron and Steel Institute, Vol. III, p. 411, 1911.) In our present case the hardness of the A. R. M. steel of 0.026 per cent of carbon is increased to that of our hardened eutectoid steel about 66 (Table VIII). After about 40 impacts at the same spot the diameter of the impressions is only about thrice that made by a single impact.

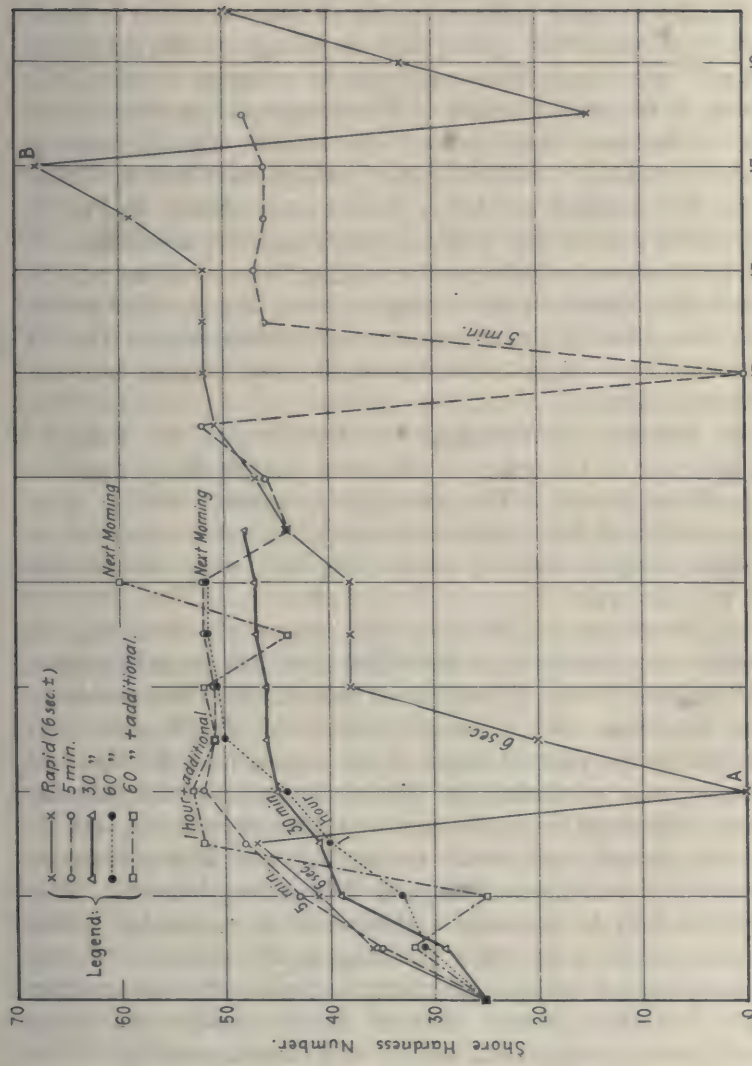


FIG 7.—Irregular Increase of the Shore Hardness of Ferrite (A. R. M. Steel) as found by Repeated Tests at a Single Spot. Detail of Fig. 5.

bottoms of all these synclinals, are to be taken liberally, because of the great difficulty, if not impossibility, of reading with accuracy a result far below the expected.

Though in many and perhaps in all cases the drop in hardness is immediate from a very high or even from the maximum hardness then reached to a minimum or even to zero, the recovery in the case of ferrite is usually spread out over several tests, for instance over twelve tests at *AB*, Fig. 5, three at *CD*, and five at *EF*, and in copper perhaps over five at *NO* in Fig. 6. The increase at *AB* is shown more clearly in Fig. 7, which shows part of Fig. 5 with a larger scale for abscissas.

The hardness of silver, after rising from 6 to 16 at the eleventh test, shows a very strong tendency to remain or return to 16 even after dropping to zero and after rising to 19. A like tendency is suggested by austenite and copper, but not clearly by ferrite.

31. *Influence of Frequency of Repetition.*—(See Fig. 7.) The influence of frequency is obscured by the sharp drops of the hardness curves. The two high-frequency curves, those for 6-second and for 5-minute intervals between tests, are on the whole steeper than the lower frequency ones, for 30 and 60-minute intervals.

32. *Discussion of Results.—The progressive hardening* is referable to the hardening of the metal about the point of impact of each successive test. The gradual retardation of the hardening in the 30-minute and 60-minute curves for ferrite, and that suggested by the general shape of the curves for austenite and for copper, may mean that, as this hardening and strengthening increase, the impact passes less and less beyond the elastic limit thus raised, and hence causes less and less permanent deformation, and hence less and less strengthening. This retardation may be increased and turned into a complete arrest or even a softening by slight cracking, as the strength of wire, after increasing from draft to draft, decreases on over-drawing.

33. *The sharp drops*, followed by a usually less sharp recovery, may perhaps be due to very slight creeping of the specimen in spite of its clamping, caused possibly by the jarring of the test itself.

One would be tempted to refer them to a slight motion

or tipping of the scleroscope by an accumulation of minor causes, such as wind impact on the building and the movement of bodies in the street and of men in the building, were it not that they show a tendency to occur only after a rather large number of tests.

A slight creeping or tipping would make the hammer strike on the sharp corner of the existing depression, so that its rebound might be far out of the vertical, and might make it strike against the sides of the vertical tube instead of rising freely. The concentration of the impact on this corner might well sink the metal there down fully to the level of the bottom of the pre-existing pit, in which case the recovery at the next test might be complete, as occurs in some synclinals in the curves of Fig. 6; or it might sink that corner metal towards but not as far as that bottom, in which case the recovery would be progressive during the following tests, as in many of the ferrite-synclinals, as that metal was forced down nearer and nearer to the pre-existing bottom level.

34. *The influence of frequency* should be interpreted in the light not only of the progressive hardening which plastic deformation causes, but also of the growth of that hardening with rest, and its great acceleration caused by gentle heating, such as no doubt occurs locally. Each blow should cause slight heating, which may well be enough to raise the temperature very materially at the immediate point of impact, even though the rise of temperature should not be detectable by our usual means, because of the rapidity with which the heat generated diffuses outwards across the mass.

In the highest frequency series, with about 6-second intervals between tests, the heating might well be cumulative to a certain degree, some of the rise of temperature caused by the first impact remaining at the time of the second. This higher temperature of the 6-second series might thus make its curve steeper than those of the three other series, for in these no such accumulation of heat is to be expected. These, then, should differ from each other only in the greater time available for aging in the lower frequency series. Hence we should expect the steepness of these three curves to be inversely as the frequency, the 60-minute series being the steepest and

the 5-minute series the flattest. That these expectations are untrue reminds us of the profundity of our ignorance of the nature and habits of plastic deformation, which makes its phenomena appear capricious, as in the saw-tooth stress-strain diagrams of Howard and of de Forest. The scleroscope seems admirably fitted for studying this deformation, because of the high frequency which it permits.

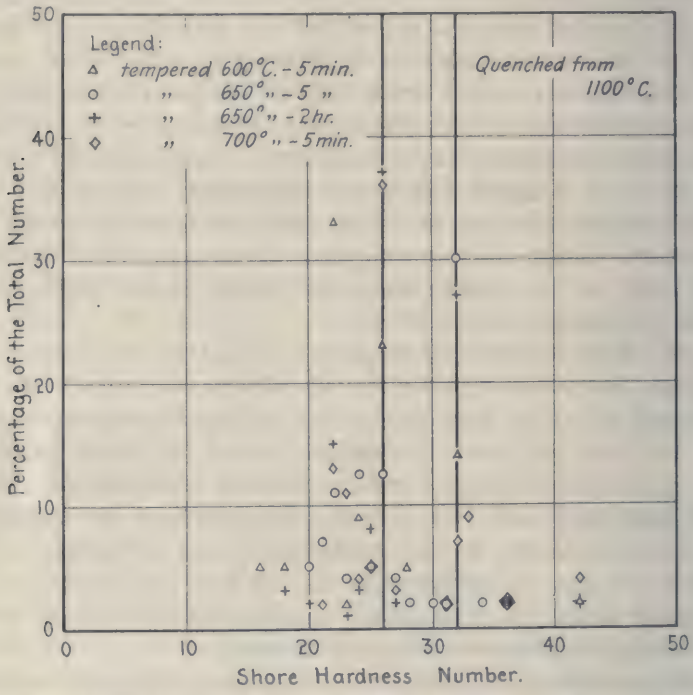


FIG. 8.—Deviations of Shore Hardness Numbers on Tempered Specimens of Eutectoid Steel, Quenched from 1100° C.

35. *The Shore Hardness of Conglomerates.*—Though as shown in Section 7, the Shore readings may be so concordant in our eutectoid steel that 49 out of 51 may fall within the limits of 23 and 26 units, yet in the same hands and even after greater experience there may be the great and apparently systematic deviations shown in Figs. 8 and 9, a very considerable fraction of the total number of observations varying from

22 to 32, or by 10 units. These figures sum up the results of 424 determinations, 204 made on eight specimens of this same steel quenched from 1100°, and 220 on eight others quenched from 800° and tempered, two of each set at 600° for 5 minutes, two at 650° for 5 minutes, two at 650° for 2 hours, and two at 700° for 5 minutes. All the 1100° quenchings were simultaneous as were the 800° quenchings, and all the temperings to given

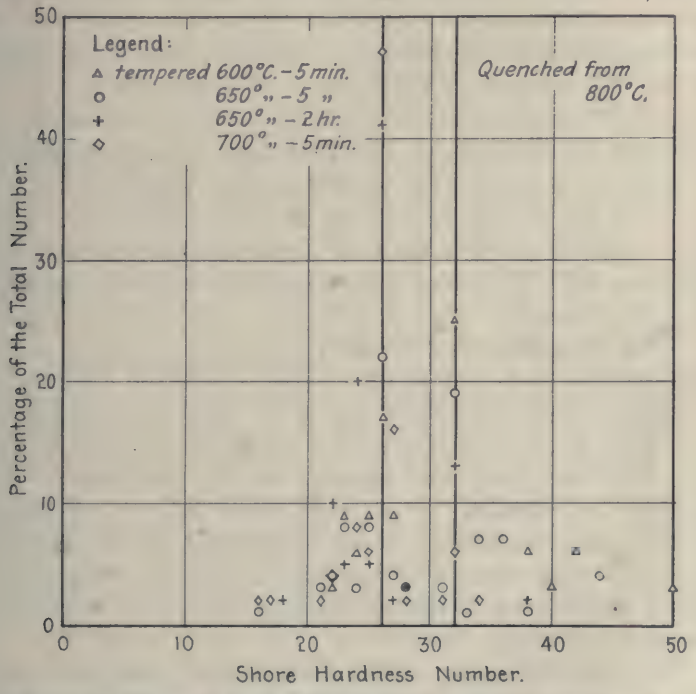


FIG. 9.—Deviations of Shore Hardness Numbers on Tempered Specimens of Eutectoid Steel, Quenched from 800° C.

temperature. Both faces of each wad were tested. Each of these four temperings is indicated by a special symbol. Ordinates show the percentage of the total number of tests which gave each specific Shore hardness. It is very striking that for both the quenching temperatures, and for all four of the temperings, the hardness is concentrated on the same pair of numbers, 26 and 32. Each of these two hardness numbers occurs far

more frequently than any of the others in each of these eight classes of specimens, representing eight different conditions. Four of the intermediate hardness numbers, 28 to 31, inclusive, occur only rarely in these results.

This concentration of the hardness results on these two numbers is not referred readily to any quality either of the scleroscope or of the eye. One might fancy that these numbers represent cases in which the point of impact comes squarely either on a little mass of cementite or on one of ferrite. Yet this explanation is not attractive, because, even if we suppose that the masses of cementite are all of one size which is most improbable, the plane of the section should cut some near their upper side or end, others near their lower end, and others at intermediate points, with corresponding differences in their snow-shoe effect. Moreover, on this hypothesis the readings greater than 32 and less than 26 are inexplicable, because there should be nothing harder than cementite squarely struck, or softer than ferrite.

That the scleroscope should give widely deviating readings when applied to a conglomerate, such as carbon steel, is only a necessary defect corresponding to its merit of detecting even small-scale heterogeneousness. Because that steel is necessarily heterogeneous, the readings of any instrument which measures the hardness of one spot must needs vary and the degree of these variations should be inversely as the size of the field which each reading covers, and directly as some function both of the number of phases present, and of the mean size of their various masses.

36. *Acknowledgment.*—This investigation has been carried out in the Metallurgical Laboratories of Columbia University, in part under a grant from the Carnegie Institution of Washington.

DISCUSSION.

Mr. R. P. Devries.—Some years ago I showed[1] that if three different kinds of Shore sclerescope hammers were used in testing steel, one a pointed hammer, one a very blunt hammer, and one Shore's own special hammer, rebounds that differed as much as 100 per cent were obtained, taking the highest rebound as the basis of comparison. Now, I believe that in those cases where the authors let the hammer drop four or five times, and then obtained no rebound at all the next time, the hammer merely stuck in the little hole which it had made. That is what I have found with a very sharp-pointed hammer. The elasticity of the metal is not destroyed, neither is the elasticity of the hammer changed; it is simply that such a hammer digs a hole for itself the first few times, and then does not rebound because there is enough friction to keep it in the indentation it has made. I suggest this as a possible explanation of why the authors in some cases obtained practically zero rebound. **Mr. Devries**

Mr. Henry M. Howe.—I think very likely that is the explanation. There seems to be also a possibility that some swaying of the building, some very slight deflection from the vertical, might cause the hammer to strike on the edge of the hole so that its rebound would not be vertical. Suppose that in spite of being clamped, there was a slight creeping. There is more or less heat developed, of course, and there might be a very slight creeping, and for some reason or other—either that reason or the swaying of the building or the passing of men or cars in the street—the hammer might strike on the edge of the cup of the previous hole. If it did that a variety of things might happen. The next blow might be strong enough to sink that corner down to the level of the previous pit, in which case the next later rebound would be the same as the previous ones. Or it might sink it part way, nearly down to the bottom, in **Mr. Howe.**

[1] "A Comparison of Five Methods of Hardness Measurement," *Proceedings*, Am. Soc. Test. Mats., Vol. XI, p. 727 (1911).

Mr. Howe. which case the next rebound would be nearly as high as the previous ones, etc.

I do not want to be understood as complaining of the Shore instrument, although I do not feel that we have fully mastered it. Under many conditions the results of our tests are extremely concordant, quite as concordant as can be had with the Brinell, machine; under other conditions, which I do not understand, they are not concordant, and I believe that others have had that experience. It is such an extremely useful machine, because it has the great advantage over the Brinell that one can explore very minute areas which cannot be touched with the Brinell. As a means of studying segregation and of exploring case-hardened pieces it has great advantages over the Brinell machine. It can probably be applied much closer to the very edge of the specimen than is possible with the Brinell machine, though of course as the edge is approached allowance for the lessening of the side support must be made.

HEAT TREATMENT OF CARBON-STEEL LOCOMOTIVE AXLES: WATER VS. OIL QUENCHING.

By C. D. Young.

SUMMARY.

It is the object of the author to show some of the characteristic physical properties of a carbon-steel forging resulting from the two different methods of treatment usually used, namely, water quenching and oil quenching.

It has been generally observed that a water-quenched carbon-steel forging will have a higher tensile strength and greater reduction of area for the same elongation than a similar forging which has been oil quenched. This, however, is not consistently borne out by the results of these tests, individually, except as to elastic limit.

Forgings having imperfections, such as segregation, forging cracks, and the like, are generally made useless by cracking during water quenching to a greater extent than in oil quenching. This peculiar property of water quenching increases the assurance of having only sound material placed in service.

HEAT TREATMENT OF CARBON-STEEL LOCOMOTIVE AXLES: WATER vs. OIL QUENCHING.

By C. D. Young.

The investigation reported in this paper was made in order to show the difference between the physical properties of a large forging quenched in water and those of a similar forging quenched in oil.

The results obtained indicate that there is an advantage in the use of water as a quenching medium, as might be expected from its physical properties. Results obtained at a large heat-treating plant, which has turned out thousands of tons of quenched-and-tempered carbon steel, indicated that no disastrous effects on the forgings are to be anticipated from the use of water as a quenching medium, providing proper care is taken in the handling of the steel throughout the process.

The forgings used for this experiment consisted of two 10-in. locomotive driving axles having a center bore 2 in. in diameter extending the entire length. Both axles were from the same melt of steel, and preliminary chemical analysis indicated the same chemical composition. One axle was treated at the Juniata Shops of the Pennsylvania Railroad Co. by water quenching, and the other axle by a steel company which makes a practice of oil quenching.

Water Quenched.—The axle marked *W* was heated to 1550° F. (843° C.), and at that temperature quenched in water at about 60° F. (15° C.). Then, in a furnace maintained at 1175° F. (635° C.), it was heated to that temperature and cooled therefrom in the air on a dry ground floor. That is, this axle, after being quenched from 1550° F. in water at about 60° F., was "drawn" to a temperature of 1175° F.

Oil Quenched.—Similarly, the axle marked *O* was quenched in a heavy oil from the same temperature (1550° F.) and drawn in a furnace to 1200° F. (650° C.), the cooling being done in the air the same as for the water-quenched axle.

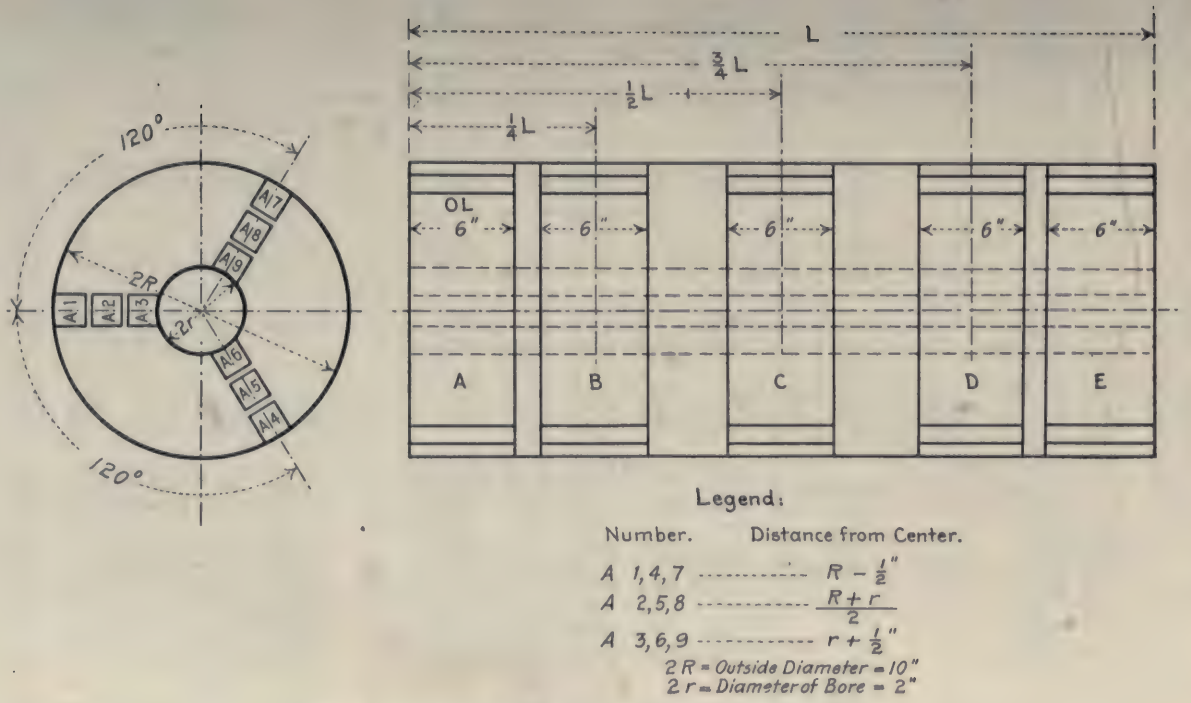


FIG. 1.—Location of Test Specimens Cut from Axles.

The object aimed at in the above treatment was to have the tension tests on both axles show an elongation in 2 in. of about 22 per cent.

After treatment, the axles were laid off for cutting up into tension test specimens as shown in Fig. 1. It will be noted in effect that out of each axle there were cut—axially and for the

TABLE I.—COMPARISON OF RESULTS ON SPECIMENS LOCATED WITH RESPECT TO AXIS OF AXLE.

LOCATION FROM AXIS = $R - \frac{1}{2}$ IN.

Specimen Marks.		Elastic Limit, lb. per sq. in.		Tensile Strength, lb. per sq. in.		Elongation in 2 in., per cent.		Reduction of Area, per cent.	
Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.
WA 1	*OA* 1	55 000	51 350	95 000	94 530	23.5	24.0	51.5	50.2
WA 4	*OA* 4	57 220	50 760	96 120	93 060	24.5	26.0	53.7	52.3
WA 7	*OA* 7	54 080	50 000	91 840	94 800	26.0	26.5	55.0	48.0
WB 1	*OB* 1	55 560	51 260	95 200	93 420	26.0	27.0	48.5	52.3
WB 4	*OB* 4	55 840	52 290	95 690	94 620	25.0	27.0	52.8	51.8
WB 7	*OB* 7	54 340	53 860	92 880	94 360	27.0	26.0	54.5	53.8
WC 1	*OC* 1	56 120	52 940	95 150	94 570	24.5	27.0	51.5	51.3
WC 4	*OC* 4	58 740	53 470	94 570	94 850	26.0	26.0	53.8	54.6
WC 7	*OC* 7	56 720	53 670	92 780	94 670	26.5	27.5	53.5	54.8
WD 1	*OD* 1	54 640	50 300	95 100	94 050	24.0	25.0	52.0	41.5
WD 4	*OD* 4	56 410	52 020	94 300	94 900	26.0	18.0	50.3	25.3
WD 7	*OD* 7	54 920	51 770	94 060	93 440	26.0	26.0	52.8	53.5
WE 1	*OE* 1	54 590	50 250	94 030	92 420	27.0	27.0	54.1	54.8
WE 4	*OE* 4	50 920	49 800	94 080	91 700	26.5	28.0	53.1	56.0
WE 7	*OE* 7	53 140	47 600	92 010	90 000	26 0	29.0	53.9	46.5
Average..	.....	55 216	51 423	94 187	93 719	25.6	26.0	52.7	49.7

LOCATION FROM AXIS = $\frac{R+r}{2}$.

WA 2	*OA* 2	56 120	46 810	97 460	93 300	25.0	24.0	49.5	45.7
WA 5	*OA* 5	53 340	48 090	97 440	94 030	26.0	24.0	47.2	39.7
WA 8	*OA* 8	49 590	48 260	97 000	93 370	24.0	26.0	39.5	45.4
WB 2	*OB* 2	50 250	49 640	96 780	95 620	23.0	25.0	43 2	43.9
WB 5	*OB* 5	49 900	50 510	95 690	92 660	25.5	25.5	46.2	41.8
WB 8	*OB* 8	46 400	47 770	94 800	93 060	24.0	27.0	46.9	45.7
WC 2	*OC* 2	48 830	47 160	95 790	92 140	24.5	27.0	44.2	49.2
WC 5	*OC* 5	52 980	48 330	95 660	92 300	25.0	25.0	43.9	47.2
WC 8	*OC* 8	49 490	49 690	94 620	93 120	25.0	26.5	45.2	46.4
WD 2	*OD* 2	53 500	48 930	96 450	93 880	24.0	24.0	41.1	43.4
WD 5	*OD* 5	57 150	47 010	95 720	91 580	24.0	26.0	42.9	46.7
WD 8	*OD* 8	47 240	48 020	92 910	91 380	24.0	27.0	41.3	47.2
WE 2	*OE* 2	51 020	50 050	95 210	92 100	24.5	25.0	46.9	47.7
WE 5	*OE* 5	49 750	47 860	94 030	90 100	24.0	24.0	42.3	49.7
WE 8	*OE* 8	50 510	47 720	92 870	89 950	24.0	27.0	43.9	47.2
Average..	.....	51 064	48 390	95 508	92 572	24.4	25.5	44.5	45.8

TABLE I.—*Continued.*

LOCATION FROM AXIS=$r+\frac{1}{2}$ IN.

Specimen Marks.		Elastic Limit, lb. per sq. in.		Tensile Strength, lb. per sq. in.		Elongation in 2 in., per cent.		Reduction of Area, per cent.	
Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.
WA 3	*OA* 3	56 120	55 740	99 050	104 050	19.0	22.5	28.6	38.5
WA 6	*OA* 6	56 410	56 120	100 100	106 800	14.0	21.0	12.8	39.3
WA 9	*OA* 9	55 840	54 150	99 960	104 500	17.0	15.5	21.8	18.0
WB 3	*OB* 3	55 840	54 640	100 500	104 800	19.0	23.0	23.9	40.9
WB 6	*OB* 6	53 370	55 230	99 400	104 900	18.0	22.5	21.6	40.1
WB 9	*OB* 9	52 310	54 220	98 800	102 300	18.0	24.0	25.5	41.1
WC 3	*OC* 3	58 000	55 690	101 300	101 400	21.0	22.0	34.5	39.1
WC 6	*OC* 6	60 460	50 760	97 780	104 000	20.0	18.0	28.3	24.9
WC 9	*OC* 9	59 300	49 360	100 400	104 900	21.5	27.0	41.7	53.8
WD 3	*OD* 3	60 100	53 600	101 500	101 200	16.5	28.0	21.0	52.3
WD 6	*OD* 6	59 700	53 740	98 580	102 300	23.0	20.0	49.2	30.3
WD 9	*OD* 9	60 920	55 120	99 600	101 600	21.0	18.0	32.5	26.1
WE 3	*OE* 3	55 840	50 760	101 000	99 240	20.0	15.0	31.5	16.2
WE 6	*OE* 6	55 630	50 760	98 070	101 500	24.5	23.0	48.7	34.0
WE 9	*OE* 9	58 990	53 260	100 200	100 950	16.0	18.0	24.5	23.5
Average.....		57 255	53 543	99 751	102 962	19.2	21.2	29.7	34.5

full length, 120 deg. apart—three radial and straight slabs of the overall thickness of a test specimen, and out of each slab five 6-in. lengths, each identified as to its original location in the axle, whether cut from the end and from which end, or whether from the center, or from midway between center and end, and from which slab; and then each 6-in. length was cut lengthwise into three specimens and their location further identified as to whether cut from the inner or outer circumference or from the middle of the wall.

As shown in Fig. 1, the identification marks for the five 6-in. lengths, in order from one end, are respectively *A*, *B*, *C*, *D* and *E*. It is also shown that the specimens from length *A*, for example, are numbered radially from the outside of the axle toward the axis, the three specimens from the first slab being numbered, as described, 1, 2, 3 throughout; second slab 4, 5, 6; third slab 7, 8, 9, respectively. The specimens from lengths *B*, *C*, *D* and *E* also have the same numbering as those of length *A*.

As already explained, the letter *W* applies only to the water-quenched axle and *O* only to the oil-quenched. For example, the markings *WC*2, *WC*5 and *WC*8 each apply to a different

TABLE II.—COMPARISON OF RESULTS ON SPECIMENS LOCATED WITH RESPECT TO LENGTH OF AXLE (See Fig. 1).

LENGTH = *OL*.

Specimen Marks.		Elastic Limit, lb. per sq. in.		Tensile Strength, lb. per sq. in.		Elongation in 2 in., per cent.		Reduction of Area, per cent.	
Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.
WA 1	*OA* 1	55 000	51 350	95 000	94 530	23.5	24.0	51.5	50.2
WA 2	*OA* 2	56 120	46 810	97 460	93 300	25.0	24.0	49.5	45.7
WA 3	*OA* 3	56 120	55 740	99 080	104 050	19.0	22.5	28.6	38.6
WA 4	*OA* 4	57 220	50 760	96 120	93 960	24.5	26.0	53.7	52.3
WA 5	*OA* 5	53 340	48 090	97 440	94 030	26.0	24.0	47.2	39.7
WA 6	*OA* 6	56 410	56 120	100 100	106 800	14.0	21.0	12.8	39.3
WA 7	*OA* 7	54 080	50 000	91 840	94 300	26.0	26.5	55.0	48.0
WA 8	*OA* 8	49 590	48 260	97 000	93 370	24.0	26.0	39.5	45.4
WA 9	*OA* 9	55 840	54 150	99 960	104 500	17.0	15.5	21.8	18.0
Average..	.....	54 857	51 250	97 111	97 650	22.1	23.2	39.9	41.9

LENGTH = $\frac{1}{4}$ *L*.

WB 1	*OB* 1	55 560	51 260	95 200	93 420	26.0	27.0	48.5	52.3
WB 2	*OB* 2	50 250	49 640	96 780	95 620	23.0	25.0	43.2	43.9
WB 3	*OB* 3	55 840	54 640	100 500	104 800	19.0	23.0	23.9	40.9
WB 4	*OB* 4	55 840	52 290	95 690	94 620	25.0	27.0	52.8	51.8
WB 5	*OB* 5	49 800	50 510	95 690	92 660	25.5	25.5	46.2	41.8
WB 6	*OB* 6	53 370	55 230	99 400	104 900	18.0	22.5	21.6	40.1
WB 7	*OB* 7	54 340	53 860	92 880	94 360	27.0	26.0	54.5	53.8
WB 8	*OB* 8	46 400	47 770	94 800	93 060	24.0	27.0	46.9	45.7
WB 9	*OB* 9	52 310	54 220	98 800	102 300	18.0	24.0	25.5	41.1
Average..	.....	52 634	52 160	96 637	97 304	22.8	25.2	40.3	45.7

LENGTH = $\frac{1}{2}$ *L*.

WC 1	*OC* 1	56 120	52 940	95 150	94 570	24.5	27.0	51.5	51.3
WC 2	*OC* 2	48 830	47 160	95 790	92 140	24.5	27.0	44.2	49.2
WC 3	*OC* 3	58 000	55 690	101 300	101 400	21.0	22.0	34.5	39.1
WC 4	*OC* 4	58 740	53 470	94 570	94 850	26.0	26.0	53.8	54.6
WC 5	*OC* 5	52 980	48 330	95 860	92 300	25.0	25.0	43.9	47.2
WC 6	*OC* 6	60 460	50 760	97 780	104 000	20.0	18.0	28.3	24.9
WC 7	*OC* 7	56 720	53 670	92 780	94 670	26.5	27.5	53.5	54.8
WC 8	*OC* 8	49 490	49 690	94 620	93 120	25.0	26.5	45.2	46.4
WC 9	*OC* 9	59 300	49 360	100 400	104 900	21.5	27.0	41.7	53.8
Average..	.....	55 626	51 230	96 472	96 880	23.7	25.1	44.0	46.8

LENGTH = $\frac{3}{4}$ *L*.

WD 1	*OD* 1	54 640	50 300	95 100	94 050	24.0	25.0	52.0	41.5
WD 2	*OD* 2	53 500	48 930	96 450	93 880	24.0	24.0	41.1	43.4
WD 3	*OD* 3	60 100	53 600	101 500	101 200	16.5	28.0	21.0	52.3
WD 4	*OD* 4	56 410	52 020	94 300	94 900	26.0	18.0	50.3	25.3
WD 5	*OD* 5	57 150	47 010	95 720	91 580	24.0	26.0	42.9	46.7
WD 6	*OD* 6	59 700	53 740	98 580	102 300	23.0	20.0	49.2	30.3
WD 7	*OD* 7	54 920	51 770	94 060	93 440	26.0	26.0	52.8	53.5
WD 8	*OD* 8	47 240	48 020	92 910	91 380	24.0	27.0	41.3	47.2
WD 9	*OD* 9	60 920	55 120	99 600	101 600	21.0	18.0	32.5	26.1
Average..	.....	56 064	51 170	96 470	96 030	23.1	23.5	42.5	40.7

TABLE II.—*Continued.*

LENGTH = *L*.

Specimen Marks.		Elastic Limit, lb. per sq. in.		Tensile Strength, lb. per sq. in.		Elongation in 2 in., per cent.		Reduction of Area, per cent.	
Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.	Water Quenched.	Oil Quenched.
WE 1	*OE* 1	54 590	50 250	94 030	92 420	27.0	27.0	54.1	54.8
WE 2	*OE* 2	51 020	50 050	95 210	92 100	24.5	25.0	46.9	47.7
WE 3	*OE* 3	55 340	50 760	101 000	99 240	20.0	15.0	31.5	16.2
WE 4	*OE* 4	50 920	49 800	94 080	91 700	26.5	28.0	53.1	56.0
WE 5	*OE* 5	49 750	47 860	94 030	90 100	24.0	24.0	42.3	49.7
WE 6	*OE* 6	55 630	50 760	98 070	101 500	24.5	23.0	48.7	34.0
WE 7	*OE* 7	53 140	47 600	92 010	90 000	26.0	29.0	53.9	46.5
WE 8	*OE* 8	50 510	47 720	92 870	89 950	24.0	27.0	43.9	47.2
WE 9	*OE* 9	58 990	53 260	100 200	100 950	16.0	18.0	24.5	23.6
Average.....		53 376	49 750	95 720	94 210	23.6	24.0	44.3	41.7

slab, but all refer to specimens cut from the water-quenched axle, from its central 6-in. length, and from the middle of the wall. The corresponding specimens for the oil-quenched axle are *OC*2, *OC*5 and *OC*8. Altogether there were 45 specimens per axle, 15 specimens from the three longitudinal planes through the axle, and 9 specimens from all three slabs from each of the five 6-in. lengths.

The test specimens were turned up to the standard 2-in. gage length, $\frac{1}{2}$ in. in diameter. The elastic limit was determined by means of a strain gage. All tests were conducted on the same 100,000-lb. tension testing machine using a machine speed of $\frac{1}{8}$ in. per minute for both the elastic limit and the tensile strength.

Results of the tension tests are shown in Table I, in which comparison is made with respect to the distance of the test specimens from the axis of the axle, and in Table II, in which comparison is made with respect to the location of the test specimens along the length of the axle. These results are summarized in Table III, in which it is shown that the average results are more nearly uniform with respect to the length of the axle than with respect to distance from the axis. This is probably due to segregation, as it was found by chemical analysis that the carbon content was not uniform throughout the section. Segregation is perhaps to be expected in the ordinary output of commercial forgings, but not to the extent found here. (See Table IV for chemical segregation.)

A comparison of the average physical properties of all test specimens from both axles (see Table III) shows that with an elongation 4.5 per cent less than that of the oil-quenched axle, resulting from the difference in treatment, the water-quenched axle gave an elastic limit 6.6 per cent greater, about the same tensile strength and nearly the same reduction of area.

Table III gives the average results from all test specimens located equidistant from the axis in each axle. The average results from the outer test specimens at the location $R-\frac{1}{2}$ in.

TABLE III.—SUMMARY OF RESULTS OF TESTS.

LOCATION FROM END.

Location from End.	Location from Axis.	Specimen Marks. Water Quenched.	Specimen Marks. Oil Quenched.	Elastic Limit, lb. per sq. in. Water Quenched.	Elastic Limit, lb. per sq. in. Oil Quenched.	Tensile Strength, lb. per sq. in. Water Quenched.	Tensile Strength, lb. per sq. in. Oil Quenched.	Elongation in 2 in., per cent. Water Quenched.	Elongation in 2 in., per cent. Oil Quenched.	Reduction of Area, per cent. Water Quenched.	Reduction of Area, per cent. Oil Quenched.
OL	....	*WA*	*OA*	54 857	51 250	97 111	97 650	22.1	23.2	39.9	41.9
L/4	....	*WB*	*OB*	52 634	52 160	96 637	97 304	22.8	25.2	40.3	45.7
L/2	....	*WC*	*OC*	55 626	51 230	96 472	96 880	23.7	25.1	44.0	46.8
3L/4	....	*WD*	*OD*	56 064	51 170	96 470	96 030	23.1	23.5	42.5	40.7
L	....	*WE*	*OE*	53 376	49 780	95 720	94 210	23.6	24.0	44.3	41.7
Average.	.....	.....	.....	54 509	51 118	96 482	96 415	23.1	24.2	42.2	43.4

LOCATION FROM CENTER.

....	$R-\frac{1}{2}$ in.	*W* 1, 4, 7	*O* 1, 4, 7	55 216	51 423	94 187	93 719	25.6	26.0	52.7	49.7
....	$\frac{R+r}{2}$	*W* 2, 5, 8	*O* 2, 5, 8	51 064	48 390	95 508	92 572	24.4	25.5	44.5	45.8
....	$r+\frac{1}{2}$ in.	*W* 3, 6, 9	*O* 3, 6, 9	57 255	53 543	99 751	102 962	19.2	21.2	29.7	34.5
....	$\frac{R+r}{2}$	Minimum permitted by A. S. T. M. Specifications		50 000		80 000		20.0		40 0	

(see Fig. 1) show the water-quenched axle to have about the same elongation as the oil-quenched axle, 7 per cent greater elastic limit, 5 per cent greater tensile strength and 6 per cent greater reduction of area.

The test specimens from the middle of the wall show lower elastic limit and tensile strength than either the outer or inner test specimens, except that the strength of the water-quenched axle at the middle of the wall was found to be somewhat higher than in the outer specimens. It is evident that this mid-region

of the section was less affected by the heat treatment. The water-quenched axle, however, shows higher elastic limit and tensile strength in this region than the oil-quenched axle, although, as already stated, the average strength of the entire section came out very closely the same for both.

The results obtained from test specimens from the inner surface of the wall are not so consistent; that is, they show a higher elastic limit and a lower tensile strength, elongation and reduction of area for the water-quenched axle.

All of the forgings tested meet the requirements of the specifications[1] of the Society, except that the elastic limit

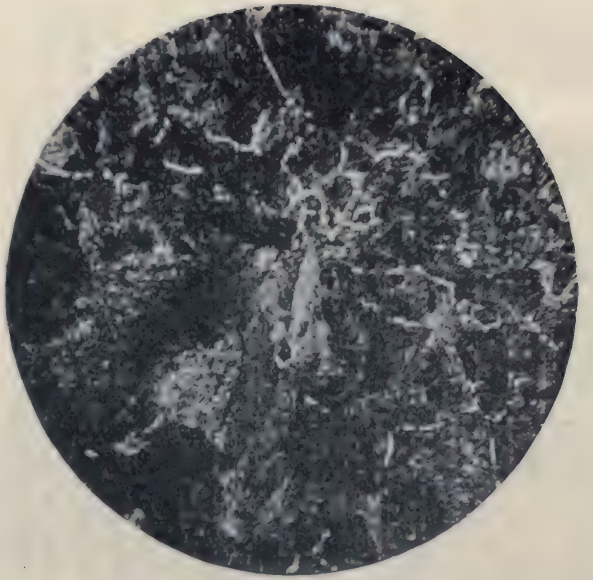
TABLE IV.—CHEMICAL COMPOSITION OF SPECIMENS.

Specimen Marks.		Carbon, per cent.	Manganese, per cent.	Phosphorus, per cent.	Sulfur, per cent.	Silicon, per cent.
Water Quenched.	Oil Quenched.					
WA 4	.....	0.53	0.56	0.019	0.031	0.159
WA 5	.....	0.53	0.56	0.018	0.030	.....
WA 6	.....	0.61	0.58	0.019	0.039	0.158
WE 4	.....	0.53	0.56	0.019	0.030	0.162
WE 5	.....	0.53	0.56	0.019	0.032	.....
WE 6	.....	0.61	0.57	0.025	0.040	0.299
WD 3	.....	0.62	0.57	0.023	0.042	0.176
.....	*OA* 4	0.55	0.56	0.018	0.030	.....
.....	*OA* 5	0.55	0.57	0.018	0.033	0.190
.....	*OA* 6	0.63	0.59	0.019	0.041	0.195
.....	*OE* 4	0.52	0.55	0.021	0.036	0.167
.....	*OE* 5	0.54	0.56	0.019	0.031	0.182
.....	*OE* 6	0.60	0.59	0.019	0.038	.....
.....	*OD* 9	0.62	0.57	0.020	0.044	0.176

found in the middle of the wall in the oil-quenched axle is somewhat low.

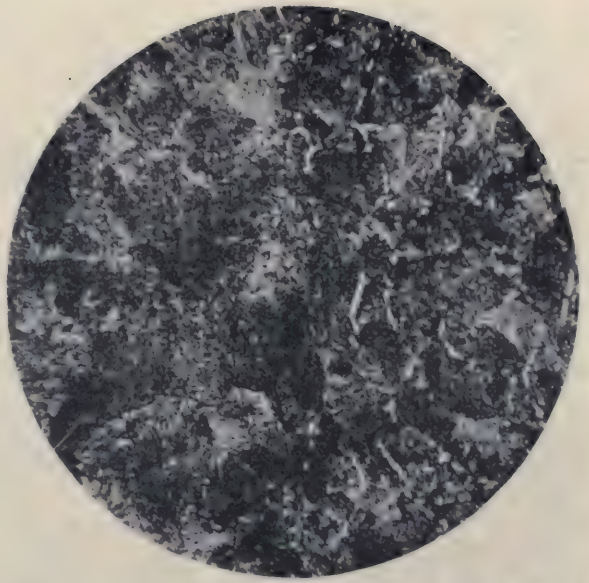
Figs. 2 and 3 show photomicrographs taken from test specimens *WE*6 and *WD*3 on the water-quenched axle. It will be noted that the physical properties of these two specimens were quite different, specimen *WD*3 showing higher elastic limit and strength but lower elongation and reduction of area. The microstructure does not afford any apparent explanation for this difference in physical properties, but in both cases indicates that the water quenching was effective in securing a good diffusion of the carbon throughout the structure, leaving only a small

[1] "Standard Specifications for Quenched-and-Tempered Carbon-Steel Axles, Shafts, and other Forgings for Locomotives and Cars," 1915 Year-Book, p. 115.



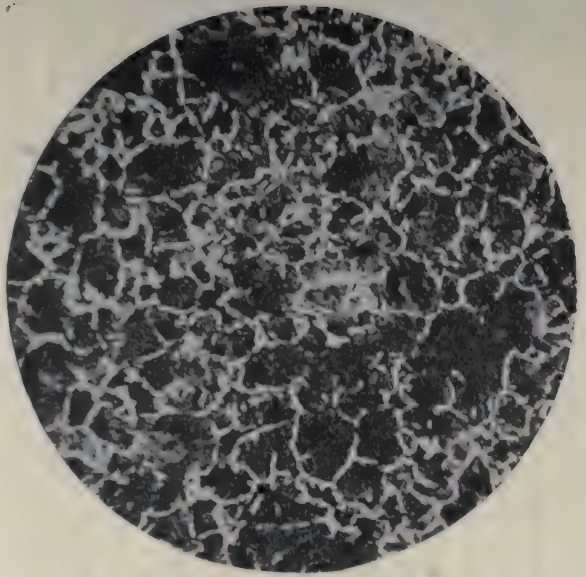
Average moderately coarse-grained, very irregular structure; sorbite with a trace of ferrite network; some spots have much ferrite and a trace of slag, while in other places there are areas in which the ferrite is completely diffused into the sorbite.

FIG. 2.—Specimen *WE* 6 (× 100).



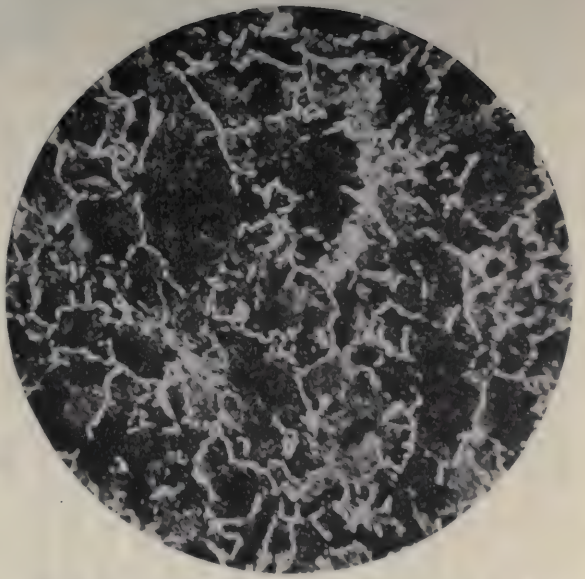
Average moderately coarse-grained, slightly irregular structure; sorbite with a trace of ferrite network; there are many areas where the ferrite is completely broken up by the heat treatment; a few tiny slag inclusions are scattered through the steel; more regular and less ferrite than *WE* 6.

FIG. 3.—Specimen *WD* 3 (× 100).



Average fine-grained, slightly irregular structure; sorbite with a slightly thick ferrite network and clusters of ferrite with a small amount of slag; structure more regular but with more ferrite than *OD* 9.

FIG. 4.—Specimen *OE* 4 (× 100).



Average moderately fine-grained, irregular structure; sorbite with an irregular ferrite network, a few clusters of ferrite and a trace of slag.

FIG. 5.—Specimen *OD* 9 (× 100).

amount of ferrite network. The carbon content in these two specimens was about the same, slightly over 0.60 per cent.

Figs. 4 and 5 show photomicrographs taken from test specimens *OE*4 and *OD*9 of the oil-quenched axle, the latter having shown higher elastic limit and tensile strength, with lower elongation. Again, no explanation of this is apparent from the microstructure, except that specimen *OE*4 indicates a lower carbon content, which was confirmed by chemical analysis. The carbon content in specimens *OE*4 and *OD*9 was 0.52 and 0.62, respectively.

The structure of the oil-quenched axle shows a fine network, which is absent from the water-quenched structure, although characteristic of carbon steel treated by the oil-quenching method.

Table IV gives the chemical analysis of representative test specimens taken from each axle.

The water-quenched axle samples, taken from the *A* end (see Fig. 1), show that the outside and midway specimens *WA*4 and *WA*5 have the same carbon content, but when compared with analysis from specimen *WA*6 of the inner wall there appears a segregation of 15 per cent. The same is true of the samples taken from the opposite end of this axle.

In the oil-quenched axle also, the same segregated condition is present, the outer and middle test specimens having about the same carbon content, while the specimens *OA*6 and *OE*6 taken close to the inner surface, show a segregation of 14.5 and 11 per cent, respectively, when compared with the corresponding samples taken from the middle of the wall.

The segregation found in both of these axles indicates a condition which increases the difficulty of securing a satisfactory treatment of the forgings, and points to the desirability of including in all specifications for forgings which are to be heat-treated, a clause to govern the allowable amount of segregation; otherwise it may be expected that extreme segregation will be found, as in the forgings here discussed.

DISCUSSION.

THE CHAIRMAN (PAST-PRESIDENT HENRY M. HOWE).— The Chairm
This paper brings home clearly to us the chief aim of most heat treatments, structural fineness or sorbitism, caused by restraining the natural tendency of the structural components to coalesce into larger and larger masses, with progressive deterioration of the quality. The micrographs at once call attention to this. In the slower oil cooling the ferrite, generated in cooling through the transformation range, has had time to assemble in part as a net work surrounding the grains of the mother austenite, and in part as independent islets within those grains, thus forming masses of important size, whereas in the faster water cooling this assembling is much more restricted. Hence the fuller assembling in the oil cooling suffices to create visibly thick and often prominent ferrite shells about a very great number of the austenite grains, now of course transformed into pearlite or sorbite, whereas the assembling in the water cooling is so much slighter that it has formed visibly thick ferrite shells about only a very few of those grains, so that most of the neighboring grains are not thus distinguished from each other, and hence a whole group of neighboring grains is taken at first for a single grain, and hence finally the grains are reported as coarser than those of the oil-cooled specimen. A sufficiently great magnification would probably show that the sorbite within these cells is decidedly finer in the water than in the oil-cooled specimen.

That the superiority of the water-cooled specimens is due to their faster cooling is further indicated by Table III, for here the faster cooling outer test pieces are better than the slower cooling intermediate ones as regards both strength and ductility in seven out of the eight averages. For our present purpose tensile strength and elastic limit may be grouped together as "strength." The inner test pieces cannot be compared fairly with the others because they are enriched by segregation, as is confirmed by the contrast between their excess of strength and their deficit of ductility.

The Chairman. The 635° C. drawing has evidently effaced in very great part the variations in each axle due to the local variations in the rate of cooling caused by the irregular convection currents. In their quenched state the forward end should be materially harder than the rear, yet, for instance, the first section of Table I shows no progressive variation from one end to the other. Again, because the local variations in hardness in the quenched state should be greater after quenching in water, the steam convection currents of which are so irregular, than after oil quenching, we might expect greater variations in strength and ductility even in the drawn specimens after water than after oil quenching. But no such difference exists, as is shown by the accompanying data, giving the average deviation from the average

Specimen.	Average Deviation from the Average.				
	Tensile Strength, lb. per sq. in.	Elastic Limit, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	
				Including all Results.	Excluding *O D* 4.
Water Quenched	1 346	1 017	0.95	1.33	1.33
Oil Quenched	1 335	1 009	1.47	5.08	3.70
Water minus Oil Quenched	+11	+8	−0.52	−3.75	−2.37

for the four tensile properties for the first section of Table I, in the water and the oil-quenched specimens respectively.

Is a drawing temperature as high as 635° really needed? Our present evidence indicates that the chemical transformation from the high to the low-temperature state, which is obstructed by the rapid cooling in quenching, becomes complete on drawing at about 400°. If so, then any further rise of drawing temperature does no good in this respect. It does harm by permitting further coalescence of the ferrite and cementite into which the completion of the transformation turns the whole, but it may offset this harm by the fuller relief of quenching stresses. Hence our natural belief is that the drawing temperature should exceed 400° by only enough to reduce the quenching stresses to harmlessness, or rather to reduce them till the benefit from their

further removal ceases to equal the harm done by the accompanying increase of coalescence. **The Chairman.**

If, for instance, the quenching stresses become thus harmless on drawing at say 500°, then 500° is the proper drawing temperature. Should this leave the strength too great and the ductility too small, it would seem better to get the needed weakening and ductilizing through smaller carbon content than through higher drawing temperature.

The question then is, with such axles is a drawing temperature as high as 635° really needed to reduce the quenching stresses properly? Can Mr. Young throw light on this?

MR. R. P. DEVRIES (*presented in written form and read by the author*).—I presume that these axles were quenched vertically without either agitating the bath or without stirring the axle itself. From work done on carbon steel water-quenched, this would seem to be a factor which cannot be neglected without detriment to the material both as regards uniformity and penetration of hardening. Le Chatelier,[1] moreover, suggests in his work done on cooling velocity in different media that agitation may be of especial value in water-quenching in promoting uniform cooling from all the surfaces, thereby reducing the possibility of deformation and fracture. **Mr. Devries.**

The author states that the specimens from the inner surface of the wall show a higher elastic limit and a lower tensile strength for the water-quenched axle. This would mean that the ratio of elastic limit to ultimate strength is greater for the water-quenched than the oil-quenched axles. It ought to be a distinct advantage for materials like axles, which are subjected in service to alternating stresses, to have this higher ratio.[2] Martens uses this ratio as a criterion for toughness. In work done on repeated-stress testing of heat-treated steel, I have found this ratio to be apparently a good criterion of endurance.

The differences encountered in the mechanical properties of the axles quenched in water or oil on the whole appear to be very small. This undoubtedly arises from the high drawing temperatures which were employed. It should not be assumed that when steel of the same composition is quenched in radically

[1] "Etudes sur la trempe des aciers," *Revue de Metallurgie*, Vol. I, p. 473 (1904).
[2] Martens: *Handbook of Testing Materials.*

Mr. Devries. different media that the results will be comparable when they are reheated to the same temperature. Results of work done along this line indicate that reheating of an oil-quenched specimen to a high temperature reduces the elastic limit but little and gives a considerable increase of ductility, whereas the water-quenched specimen appears to gain in ductility at a sacrifice of elastic limit. At first glance this might seem to be a point in favor of oil quenching, but it should be considered that the oil-quenched specimen does not reduce as much proportionately to the water-quenched in elastic limit for high temperatures of reheating because a high elastic limit was not obtained on quenching. The microstructures typical of both oil and water quenching seem to show that the heat treatment as a whole was little more than a grain-refining process with a slight difference i nfavor of the water-quenched axles.

I should like to ask one question. Would Mr. Young expect these axles to show good endurance in service where there is some impact, and also alternating stress?

Mr. Unger. MR. J. S. UNGER (*presented in written form and read by the author*).—In the third paragraph of the summary of Mr. Young's paper we are told that segregation results in cracking on water-quenching, and in the last paragraph of the paper that these axles had "extreme segregation." The question naturally follows: Why did the water-quenched axle not break when the prescribed conditions were present?

If segregation is to be made responsible for cracking on quenching, how can we water-quench a piece of case-hardened steel or of deeply cemented steel without cracking? In these materials we have produced artificial segregation of the carbon many times greater than the slight segregation shown in Table IV, yet the case-hardened piece does not break.

As the carbon in steel increases it becomes more sensitive to rapid thermal changes. An axle of 0.55-per-cent carbon will stand more thermal abuse than one of 1.00 per cent. It is practically impossible to water-quench a 10-in. hollow axle of 1.00-per-cent carbon without breaking it into several pieces.

Small articles of high-carbon steel for tools, valves, milling cutters, saws, etc., which must be hard, are quenched in oil, not because they are segregated, or have forging cracks, or

surface defects, but because the rapid cooling in water induces strains which rupture the material, even though it is homogeneous throughout. Mr. Unger.

Section has a great influence on the cracking of steel when quenched in water. For instance, the crops cut from rails at the hot saws, when thrown into water, invariably crack at a point between the head and web. This does not happen if the piece be quenched in oil.

Size of object must also be considered in quenching in water. No large forgings, such as a 24-in. engine shaft, when of a grade of steel similar to the axles under examination (0.55 to 0.60-per-cent carbon), are ever quenched in water. The manufacturer may find it possible to water-quench axles up to a certain size, say under 6 in., but most of them do not quench the axles in water when they are larger than this size.

In a comparative test of the kind covered by the paper, it would appear that every factor which might influence the results, except the quenching medium, should be eliminated. I find the only common properties were that they were of the same heat of steel and had the same chemical composition. Whether they were forged by the same firm, or at the same temperature, is not stated, although this has some influence.

To obtain truly comparative results, the axle must be from the same ingot and as nearly as possible from the same location in the ingot. As this is impossible, an axle should be cut transversely at the center, and each half treated at the same time and in the same furnace. To treat an axle in a furnace at one place and a second axle at another in a different furnace, using, perhaps, a different fuel, rate of heating, and manner of applying the heat for both the quenching and drawing back, does not appear to be truly comparative when the slight differences shown by the tables are to be studied.

Nothing is said about the oil used for quenching. Water may be assumed to be practically uniform in action when used at the same temperature, but different quenching oils differ very materially. Investigations by Matthews and Stagg and by the Houghton people of vegetable, animal and mineral oils have shown differences in the hardness of the same steel quenched in

Mr. Unger. various oils of about 15 per cent, when all other conditions were the same.

We all know that water will quench more rapidly than oil, consequently, we expect a greater hardness in the steel which has come in contact with the water than with the oil. Such being the case, a water-quenched steel is harder on the surface than an oil-quenched one.

When the same physical properties are desired, the draw-back temperature after water quenching in regular axle practice is about 25° C. higher than for oil; but in this test the reverse is true; the oil-quenched axle was drawn back at 15° C. higher than the water. A study of the tables shows a higher elastic limit on the water-quenched axle, but a pronounced proportional change in the reduction of area from the outside to the center of both axles. The reduction of area, to my mind, is a better criterion of toughness in an axle than the elongation. Both axles show a falling off in reduction of area as the test approaches the center, but the water-quenched axle falls off much more rapidly.

I believe any comparison of results of any particular locality unfair, as the entire axle goes into service; consequently, the averages of all results should be compared. This is done in Table III, where it is found that the elastic limit is higher in the water-quenched axle, but the elongation and reduction of area are higher in the oil-quenched axle.

The American Society for Testing Materials' 1915 specifications for 10-in. axles calls for a test at the middle of the radial thickness to show 50,000 lb. per sq. in. elastic limit, 85,000 lb. per sq. in. tensile strength, 19.5 per cent elongation and 37 per cent reduction of area. The second part of Table I shows that seven of the water-quenched tests and thirteen of the oil-quenched tests failed on elastic limit, or could not be accepted by the Society test.

The results do not show superior qualities in one axle over the other, nor has it been proven that a segregated water-quenched axle will crack. To make a test of one axle of each kind and imply that water-quenching should be adopted, is sweeping in character and hardly borne out by the evidence presented.

MR. L. R. POMEROY.—If not out of order, I should like to ask if it is possible to incorporate in this paper the physical properties of the axle previous to quenching in either water or oil, as the recorded data are, for the most part, based on the initial bar prior to thermal treatment. These would afford a basis of comparison of the results of the various methods of treatment, and also serve as an aid in comparing results where the cooling medium has a great deal to do with the structure to be obtained. **Mr. Pomeroy.**

MR. C. D. YOUNG.—Replying to Mr. Devries' statement in reference to the difference between the ultimate and elastic limits found between water and oil-quenching, I think that difference is generally found when the ordinary quenching methods are used. It is rather the opinion, and Mr. Fry has some data to show that beyond any doubt, that it is the time of cooling rather than the medium that has the greater effect in producing the physical properties that may be desired normally. As to the way these axles were quenched, the water-quenched axles were immersed vertically so that the water came up through the hollow boring; in fact, when the axle first goes into the water bath, a geyser is produced which goes up about 40 ft. through the axle. The oil-quenched axle, on the other hand, is immersed horizontally. The axle is removed from the water in about six minutes, when it is at about 550° F. temperature, and is then taken to the annealer; whereas in oil quenching the axle remains 25 to 30 minutes before it is removed, and it is then at the same temperature as the oil. As previously stated the difference between the effects of the water and oil is in the time rate of cooling, and the cooling rate has, I believe, more effect than the cooling medium itself, although the specific heat of the latter has some effect. Mr. Devries has asked whether we would expect better results in resisting dynamic shock from axles so treated as compared with annealed axles. **Mr. Young.**

MR. DEVRIES.—I do not want to question you as to your commercial practice, but I just want an expression of opinion as to whether these axles would stand up under service conditions? **Mr. Devries.**

MR. YOUNG.—The answer to that question is, yes. The A.S.T.M. forging specification represents what is accepted as good commercial practice in the use of heat-treated forgings. Perhaps a lower ductility, as low as 15 per cent, would be acceptable, and this would be accompanied by a higher elastic limit. **Mr. Young.**

Mr. Young. We do not know anything, however, about the results this would lead to in service, and consequently we have misgivings. The Society and the users have, therefore, held the elongation at almost the same as required for annealed forgings. The service of these axles has been excellent. They are sufficiently ductile to stand the heavy dynamic shocks of railroad service, and when one considers the advantages of having practically an annealed forging with an increase of 50 per cent in the elastic limit the tremendous gain to the designer in lightening the parts will be appreciated. It is for this reason that the road I am connected with is using heat-treated forgings to such a large extent.

Mr. Howe asks why we use such a high drawing temperature. That is used in order to produce the required physical properties, including elongation, with steel of the chemical composition we may happen to have, based upon experience in handling the heat-treating plant. If the carbon is low, it is very difficult to get the elastic limit requirement of 50,000 lb.; whereas if the carbon is high, the steel must be drawn high in order to meet the elongation requirements. Thus, in the operation of the plant regard must be had, in the way just indicated, to the chemical composition and the physical requirements in the specifications, with a view of insuring the largest possible percentage of acceptances. In other words, the chemical composition being prescribed, the drawing temperature must be selected accordingly in order to meet the physical requirements, which are also prescribed. Whether this procedure is best or not in so far as the product is concerned, is a different question. I am convinced, however, from our own experience that if we should lower the carbon limit, keeping the range about the same, using, for example, 0.35 to 0.50 per cent for 10 or 11-in. forgings, it would be exceedingly difficult to secure many acceptances on account of the elastic limit, which would be about 45,000 to 48,000 lb., that is, just under the acceptance limit. Thus, the elastic limit requirement would have to be lowered if the carbon limit were lowered.

Referring to Mr. Unger's discussion, I can find nothing in the paper to indicate that we advocate water quenching as opposed to oil quenching. We point out some of the desirable features of the water quenching based upon experience, and these, as I explained when I presented the paper, did not refer

to the two particular axles discussed. As regards the conditions not being uniform: these were made as nearly uniform as we could obtain with the steel; not being makers of steel, it was only possible for us to use it as we found it. The axles were forged as nearly alike as possible, although the manufacturer took no particular precautions, I believe, other than he would with commercial work in general. We made the test to obtain roughly the effect of oil versus water quenching in one manufacturing plant as compared with another. The other conditions were made as nearly uniform as practicable. The suggestion of Mr. Unger that we split the axle would not have met with favor with us, because we would have felt that it would not be an axle if it were split, and the effect would not have been the same because one manufacturer was water-quenching vertically and the other was oil-quenching horizontally. I fail to see why we should have taken the average of all conditions; the fiber stresses are not uniform in service, and the physical properties of the test specimen taken near the point of maximum fiber stress would interest me much more than the average of the whole piece. Mr. Young.

Mr. Pomeroy has asked for the physical properties of the original forgings. These were not taken in hollow-boring the axles. For a 2 or 3-in. hole, it is necessary to anneal the forging in order to save the cutters and to save time in the machine work. I should expect these forgings to have had practically the same ductility as after the treatment, but an elastic limit at least 45 per cent lower, and an ultimate strength about 10 or 15 per cent lower than for the finished heat-treated forgings. Thus, the real change by heat treatment is the raising of the elastic limit, which is what we are after in the heat treatment of our forgings.

MR. L. H. FRY.—There has been from time to time a good deal of discussion regarding the relative merits of oil and water for quenching forgings, and on hearing the title of Mr. Young's paper I hoped that he was going to give us some figures which would help us towards a decision. This is not the case, as the physical results quoted are not representative of the best practice in either oil or water quenching. From the last paragraph of the summary of his paper, Mr. Young appears to be so strong an advocate of water that its tendency to crack forgings, which has generally Mr. Fry.

Mr. Fry. been considered a disadvantage, is put forward as providing a safeguard. Water is excellent for many purposes, but for the quenching of forgings I prefer oil. Whatever medium is used there is a tendency for the forgings to crack on being quenched and it seems to me desirable to use a medium which will reduce this tendency to a minimum; and then to submit all quenched-and-tempered forgings to a proof test, for the elimination of any forgings which may have cracked.

So much for the effect of the quenching medium on the solidity of the forgings. Now consider the physical properties produced by quenching. These properties in any given steel are dependent on the rate at which the heat is extracted and apart from this are not dependent on the medium by which the heat is extracted. Really, in discussing physical properties it would be more logical to make a comparison between steel cooled in ten minutes and steel cooled in thirty minutes rather than to compare water-quenched and oil-quenched steel. This is true because the rate of cooling and consequently the physical properties may be influenced more by the size and shape of the piece, than by the medium in which it is quenched. A test piece an inch in diameter and a few inches long will cool in air more rapidly than a locomotive axle will in oil and thus the physical properties obtained by air cooling may be better than those obtained by oil-quenching.

The circulation of the medium is also of great importance in determining the rate of cooling. In any given quenching medium the rate of cooling can usually be accelerated by a forced circulation of the medium. As an example of the improvement in physical qualities which can be effected by this means, the following figures are given:

Axle.	Elastic Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.
A.....	49 500	95 000	20.5	43.0
B.....	69 000	105 000	21.0	42.0

The two axles were of the same design, made from the same steel, and were both quenched from the same temperature in the same medium—a solution of one part of cutting compound to three parts of water—and were both drawn at the same temperature.

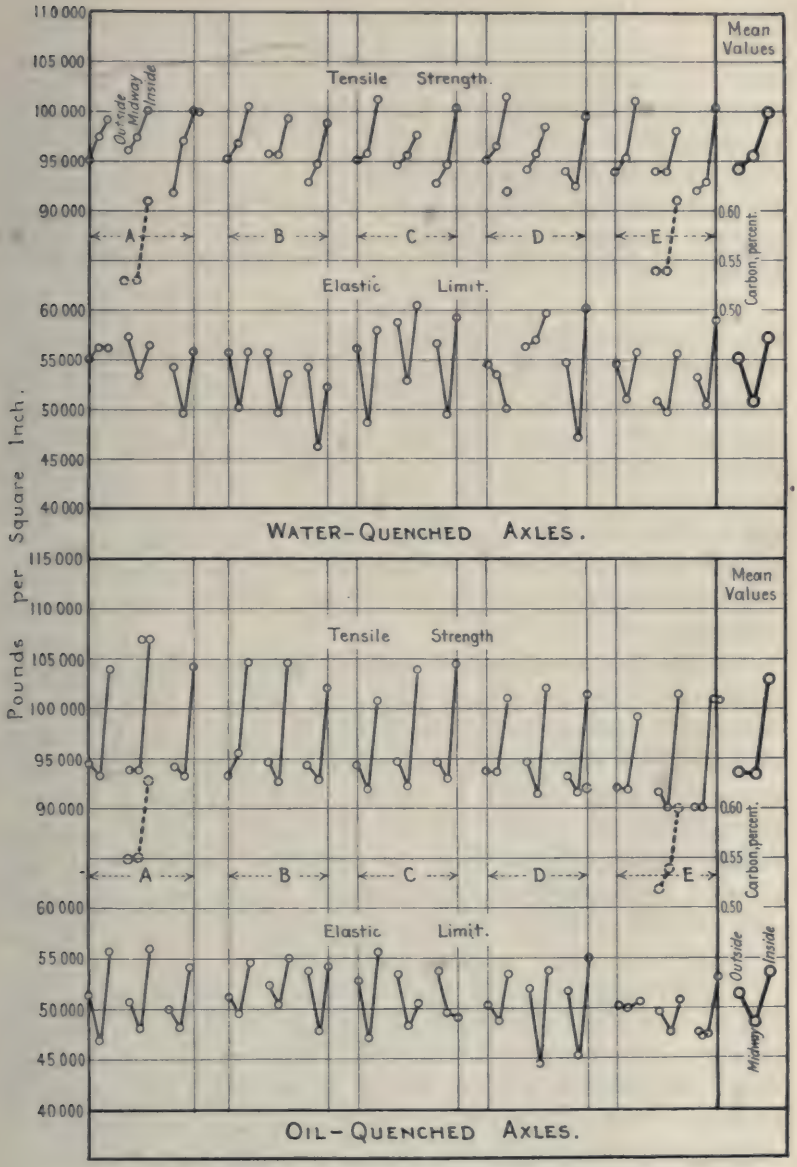


FIG. 1.—Comparison of Oil and Water Quenched Axles.

Mr. Fry. For axle A the medium was at rest, while for axle B it was forced into fairly rapid circulation by a jet of compressed air in the tank. These results are taken from a series of experiments made with various quenching media in which the axles were arranged so that a pyrometer could be inserted into the axles and the fall in temperature recorded throughout the process of quenching. The time taken to cool from 1450° F. to 700° F. was about 16 minutes for axle A and about 9 minutes for axle B. The improvement in physical properties due to this increase in the rate of cooling is obvious.

To return now to Mr. Young's paper. I have plotted in Fig. 1 the elastic limit and the tensile strength for all of his tests. The tests from each longitudinal section, *A*, *B*, *C*, *D*, and *E*, are grouped together, and the three tests taken on the same radial line are connected by lines, and are plotted in order, the first point representing the test from the outside of the radius, the second point the test midway of the radius and the third point the test at the inside end of the radius. The mean values are plotted in the same way at the right-hand side of the diagram. The percentages of carbon given in Table IV of the paper are also plotted.

In the case of the elastic limit the variation from outside to inside is the same for both quenching media, a drop from the outside to a midway point, and then at the inside a rise to a figure slightly higher than that of the outside. In the case of the tensile strength there is a slight, but clearly marked difference between the oil and the water. In the oil-quenched axle the tensile strength drops slightly from the outside to midway, and then shows a considerable rise to the inside. In the water-quenched axle the rise in tensile strength is continuous from the outside through the midway point to the inside.

Mr. Young attributes these differences to carbon segregation, but I do not think that this is an adequate explanation. I think that the difference is due to a considerable extent to stresses set up by the different rates of cooling on the inside and outside of the axle. This suggests that if the stresses due to differences in cooling rates have the effect on a bored axle they will have a very much greater effect on an unbored axle. It is very desirable to bore all locomotive driving axles before quenching.

MR. YOUNG.—What Mr. Fry has just said in reference to the results is, I think, entirely in accord with our general experience, in that the lowest results are to be looked for between the inner or bored surface and the outer circumference of the forging, and that the time rate of cooling to a definite temperature or carrying through the critical range for a given percentage of carbon, is a much more important factor in heat treatment than the actual medium that is used. The medium is merely a conveyance for carrying away the heat from the forging during the quenching period. Mr. Young.

THE CHAIRMAN.—To push this question a little further, doesn't Mr. Young think that the high elongation which those specifications call for is a superstition, a relic of barbarism, when he remembers how low the elongation is in the patented wire which is used under pretty strenuous conditions with a tensile stength of 400,000 lb. per sq. in. and does not break. Why then, we begin to wonder and ask ourselves, do we call upon these materials, these objects like axles, to exhibit a property-like ductility in the tensile test which they are never going to be called upon to show in service? Why test them for a quality which apparently is useless in service? I do not say it is really useless, but it is apparently useless. We do know that arts remain in a state of barbarism for decades and then we wonder why we were so foolish for so long. We may be foolish at the present time in this matter or we may not, I cannot tell. The Chairman.

I will also ask Mr. Young whether horizontal quenching is not radically wrong, whether it does not throw undue stresses on the entrance side by causing much more rapid cooling there than on the upper side? As regards the agitation, how is that brought about? If it is a spinning, then one would fear that, unless the axis of the spinning was strictly concentric with the axis of the piece, it would exaggerate the irregularities brought about by the convection currents. If the circulation is not strictly concentric, then we have one side cooled off more rapidly than the other side, the side perhaps which the medium passes more rapidly. Also, isn't it rather a question of the stresses than of the rate of cooling between the outside and the inside? It looks at first as if the outside must cool considerably faster than the inside, but the stresses on the outside are the reverse of the stresses on the

The Chairman. inside, and as we know, those stresses have a very profound influence on the nature of the transformation. The transformation involves, first an expansion followed by a compression, and the nature of the stresses determines the point at which the transformation shall be arrested.

Mr. Young. MR. YOUNG.—As to horizontal quenching, I have had no experience with it; we have always used vertical quenching and believe it is correct in theory. It has worked out satisfactorily in practice and we have never tried the horizontal method. The circulation in the horizontal method is obtained, I believe, by running the oil in and out of the tank very rapidly. I think that the point raised as to one side getting the cooler medium, and the other side the hotter would not be of great importance in the circulating problem for the reason that there are in general seven or eight large forgings in a quenching charge, and these have more or as much effect as the difference in the temperature of the oil in the bath. As regards the lower elongation, my views are those of the test department, whereas the mechanical engineering department specifies the ductility they desire. They are the people who fix the fiber stresses in the design, and who feel they should have proper protection as to ductility, that ductility being determined by long experience with pieces which have failed. I think it would take a long time to get the mechanical engineers of this country to accept forging steel with an elongation as low as 15 per cent. If they did use it, the steel might be found to meet the service conditions much better than that having lower elastic limits.

Mr. Fry. MR. FRY.—I should like to speak on two questions which Mr. Howe has raised. The first is the possibility of using steel with a high elastic limit and a comparatively low elongation. We have recently had occasion to make some forgings for exceptionally severe service with rapidly alternating stresses. The first forgings were of ordinary quenched-and-tempered steel with an elastic limit of about 50,000 lb. per sq. in. and an elongation of about 20 per cent. These failed to stand up and were replaced by others of the same size, but made of high-carbon steel with an elastic limit of about 80,000 lb. and an elongation of only about 15 per cent. These forgings have proven perfectly satisfactory. The high elastic limit has enabled them to resist the shocks in spite of the lower elongation.

Mr. Fry.

The second question is the possible variation in quality between two sides of an axle quenched horizontally. We have made a number of tests and have always found that horizontally quenched axles give substantially uniform results at all points on a circumference. The results in Mr. Young's paper illustrate this. The water-quenched axle was quenched vertically, while the oil-quenched axle was quenched horizontally, but the results of the tensile tests are equally uniform in both cases.

RECRYSTALLIZATION AS A FACTOR IN THE FAILURE OF BOILER TUBES.

By A. E. White and H. F. Wood.

SUMMARY.

Frequent replacements of tubes in certain boilers that were run under high rates of evaporation for days at a time during the winter months, so interfered with efficient operation that the company in control of these boilers requested a detailed study of the influences affecting the properties of tube metal. This paper presents simply one phase of the subject, a phase dealing with conditions producing crystal growth.

A careful summary of the researches which have been made in an attempt to explain the exact phenomena of recrystallization on deformed low-carbon steels, is presented. The laboratory methods employed in the present investigation are briefly described, and the results secured are given.

It has made possible the formulation of a hypothesis giving the time-temperature relationship affecting crystal growth in deformed low-carbon steel at temperatures lying between 675 and 550° C. Expressed as a geometrical progression, such a formulation is:

$$T = a,\ ar,\ ar^2,\ ar^3, \ldots\ldots ar^{n-1}$$

where T = time in minutes for every fall of temperature of 25° C., starting at 675° C. as a maximum; $a = 8$ (a numerical constant); and $r = 3$ (a factor). Experimental evidence within the given ranges of temperature has shown this hypothesis to be correct. For instance, heatings for eight minutes at 675° C. and for nearly 35 hours at 550° C., respectively, were required before a noticeable beginning of crystal growth in previously deformed low-carbon steel could be detected. On the assump-

tion that the hypothesis would hold equally well for all lower temperatures, it is evident that at atmospheric temperatures thousands of years would be necessary for the development of crystal growth in deformed iron having a low carbon content.

The investigation has also resulted in showing that, due to the combined effects of high rates of evaporation and a slight scale deposit, the temperature attained by some of the tubes reached a high degree, so high in fact, that if the metal had previously undergone deformation at temperatures below its A3 (850° C.) point, and if the time during which it remained at such temperature was sufficient, coarse crystals resulted. These coarse crystals produce a marked deterioration of the good properties found in the tube metal, and contribute their share to a trouble spoken of as "bagging."

To offset such a state of affairs, it is held highly desirable to free the water from all scale-producing minerals. In the plant in question this feature is receiving due attention. It is furthermore suggested that in the production of boiler tubes, a serious attempt be made to secure a metal which, when in state of strain, will not be subject to grain growth. It is felt that a tube with a higher carbon content than that now generally introduced may possibly fulfill such a condition. At all events, the matter is about to undergo a trial.

RECRYSTALLIZATION AS A FACTOR IN THE FAILURE OF BOILER TUBES.

By A. E. White and H. F. Wood.

This study was started in an attempt to explain the failure of boiler tubes at a plant where very heavy loads were carried for a number of weeks at a time. The metallographic examination of tubes showing incipient failure, revealed coarse crystals at the surface of the tube exposed to the high temperature. These failures also universally occurred in the front row of tubes where the temperature was highest and only on the side exposed to the flame. The question then naturally arose as to the conditions under which the coarse crystallization could have resulted. The effect of time, temperature and stress, therefore, on boiler-tube metal was accordingly investigated.

The Recrystallization of Iron.

The first important research on the recrystallization of deformed iron was by J. E. Stead[1] in 1898. He found that in low-carbon steel which had been forged below its critical temperature, a growth of the ferrite crystals took place upon annealing below 850° C. The growth was very slow at 500° C., but by heating the metal for a few hours at 700° C. he succeeded in producing an exceedingly coarse crystallin structure.

A short time later the results of a number of researches relating to the recrystallization of cold-drawn iron and steel wires were published. Among the most noteworthy investigators on this subject may be mentioned Longmuir[2] and Goerens.[3] The latter showed that the ferrite in cold-drawn iron and steel mainly recrystallized between 525 and 600° C., the

[1] "The Crystallin Structure of Iron and Steel," *Journal*, Iron and Steel Institute, 1898, No. 1, p. 145

[2] "Some Aspects of Wire Drawing," *Journal*, Iron and Steel Institute, 1912, No. 2, p. 142.

[3] "On the Influence of Cold-Working and Annealing on the Properties of Iron and Steel," *Journal*, Iron and Steel Institute; Carnegie Scholarship Memoris, Vol. 3, 1911, p. 320.

process consisting in the transformation of the elongated ferrite crystals into crystals of the normal allotrimorphic shape.

About the same time Charpy[1] and Le Chatelier[2] took up the study of locally deformed iron, the deformation being made through the agency of the Brinell ball hardness machine. An annealing of these locally deformed specimens below the critical temperature resulted in the development of abnormally large crystals. Their researches brought out the fact that these abnormal crystals only appeared in a certain part of the deformed area. Later, Sauveur[3] introduced what he called the "critical range of stress." He means by this that if the stress present in the metal is below a certain minimum limit or above a certain maximum limit, there will be no crystal growth upon annealing at a temperature below the critical point. It is the opinion of the authors that the limits of this range of stress vary with the composition of the metal.

The first important research on the influence of temperature on the recrystallization of deformed iron was by Robin.[4] He pointed out that in soft steel which had been deformed during the process of rolling, the crystal size reached a maximum upon annealing at about 725° C. and then slightly decreased until the A3 point was reached, at which temperature all existing changes were destroyed.

The most recent investigation of recrystallization was by Chappell.[5] In this valuable research he made an extensive study of both cold-drawn and locally deformed irons. He divided the recrystallization process into three stages: (1) the appearance of granular markings on the deformed ferrite crystals denoting disintegration; (2) the appearance of distinctly visible but small independent crystals; and (3) the growth of these small independent crystals. He carried Sauveur's[3] "critical range of stress" idea a little farther by distinguishing the pres-

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[1] "Sur la maladie de l'écrouissage," *Revue de Metallurgie*, Memoirs, Vol. 7, 1910, p. 655.

[2] "Notes de Métallographie," *Revue de Metallurgie*, Memoirs, Vol. 8, 1911, p. 637.

[3] "Notes on the Crystallin Growth of Ferrite below Its Thermal Critical Range," *Proceedings*, Int. Assn. Test. Mats., Sixth Congress, 1912, Vol. 2.

[4] "Recherches sur le développement des grains des métaux par recuit aprés écrouissage," *Revue de Metallurgie*, Memoirs, Vol. 10, 1913, p. 752.

[5] "The Recrystallization of Deformed Iron," *Journal*, Iron and Steel Institute, 1914, No. 1, p. 460.

ence of three zones at each point of deformation; (1) the zone of maximum deformation immediately surrounding the depression; (2) the zone characterized by very large crystals in which the deformation had not been as severe; and (3) the zone directly below the last one which had suffered little, if any, deformation, and in which no growth had taken place. He conclusively showed that the higher the degree of deformation for the second zone, the lower the temperature of recrystallization. Also, he demonstrated that the presence of carbon reduced the size of the gross crystals produced.

OBJECT OF THE PRESENT RESEARCH.

The object of the present research has been as follows:

1. To obtain a curve indicating the relation between the time and temperature governing crystal growth at temperatures below the critical range in a deformed low-carbon steel; and

2. To make a systematic study of the conditions existing where low-carbon steel is used at moderately high temperatures, after having been subjected to sufficient stress to result in permanent deformation, and to ascertain if the recrystallization phenomena, known to exist under such conditions, have any influence upon the possible failure of the metal.

To accomplish this, a steel of the following chemical composition was used:

Carbon	0.113 per cent
Manganese	0.479 "
Phosphorus	0.023 "
Sulfur	0.035 "
Silicon	0.023 "

EXPERIMENTAL METHODS.

Preparation of Samples.—All samples were first annealed for three hours at 1000° C. in an atmosphere of hydrogen. This was done in order (1) to eliminate the influence of every possible strain, (2) to make certain that in all specimens the same microconstituents, and the same percentages of them, were present, and (3) to increase crystal size in the samples so that

they could easily be observed and measured. Each specimen was then subjected to a local deformation by means of a 5-mm. Brinell ball under a pressure of 800 kg. This pressure was selected after due experimentation, because it was of such an intensity as to always cause the zone of gross crystallization to occur directly below the depression. The samples were then cut through the center of the depression so that the structure of the metal lying directly below could be observed and studied. A sample so prepared is shown in Fig. 1.

The specimens were then placed in an annealing furnace and heated at the desired temperature for the required time. If the annealing lasted more than ten minutes, each sample was put into a glass tube, which was then evacuated and sealed in order to prevent severe oxidation of the sample during the heat treatment.

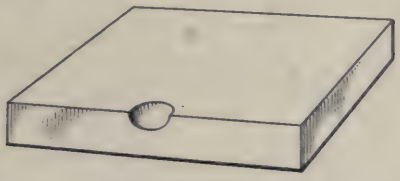


FIG. 1.—Sample Cut through Center of Depression from Brinell Ball.

On removal from the heat-treating furnace, the surface of the specimens lying in a plane through the center of the depression was polished and etched with a 5-per-cent solution of nitric acid in absolute alcohol. Two photomicrographs were taken of each sample; the location of the first was directly below the zone of maximum depression, and that of the second was from some section representing the average structure of the unstrained metal.

Furnace used for Heat-Treatments.—The type of furnace used for heat treating the samples is illustrated in Fig. 2. The furnace was so insulated that it was possible to control the temperature very accurately. The coiled nichrome-wire heating unit was covered with a thin layer of refractory cement made from a mixture of fire clay, silex and sodium silicate and this, in turn, was covered with a layer of shredded asbestos.

In case the nichrome winding should burn out, such a method of packing allowed a rapid replacing of the whole heating unit. A platinum, platinum-rhodium thermocouple was used for the temperature measurements. The arrangement of the furnace is shown in Fig. 3.

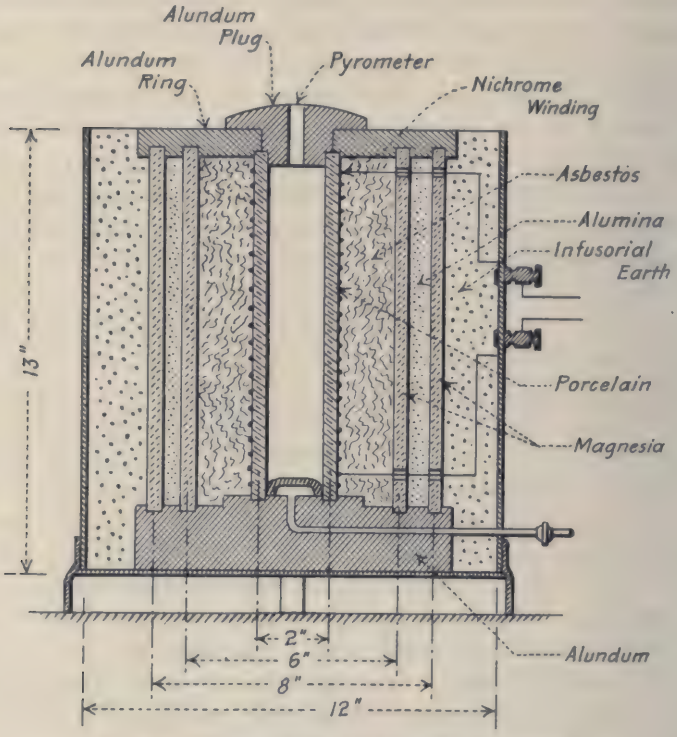


FIG. 2.—Furnace Used for Heat-Treatments.

METHODS FOR DETECTING CRYSTAL GROWTH.

Macroscopic.—The presence of crystal growth can be seen easily with the naked eye by examining the surface of a sample during the process of etching. This is observed by first covering the polished surface with the etching solution and then rotating the sample in order that the light may be reflected from the surface at different angles. Some one of these angles will never

fail to show evidence of a change if there has been even the faintest trace of recrystallization.

Mechanical.—For this method two photomicrographs from each sample, magnified in each case exactly 100 diameters, were taken. The prints of these photomicrographs were so made as to bring out very distinctly the crystal boundaries. On each print an area ranging from 6 to 10 sq. in. was traced by a line following the crystal boundaries. This area was then accurately measured by means of a planimeter; the average of three readings being considered in each case as the true area. The micro-

FIG. 3.—Arrangement of Furnace Used for Heat Treatments.

constituents of an annealed hypoeutectoid steel, that is, one containing less than 0.86 per cent of carbon, are ferrite and pearlite or sorbite. The steel under consideration contained 0.113 per cent of carbon and by means of the following equations the actual area of free ferrite in the area measured by the planimeter was computed:

$$F+P=100$$

$$\frac{0.86P}{100}=C$$

where F = area of ferrite in square inches; P = area of pearlite in square inches; and C = percentage of carbon.

The number of ferrite crystals present in each area, divided by the area, gave the number of crystals per square inch. It is self-evident that the number of ferrite crystals is the criterion. A decrease in the number of those found in the zone of maximum depression after heat treatment, as compared with the number

TABLE I.—TIME-TEMPERATURE RELATIONSHIPS AFFECTING CRYSTAL GROWTH IN DEFORMED LOW-CARBON STEEL.

Sample No.	Temperature, deg. Cent.	Time at Heat, min.	Area, sq. in.	Pearlite, per cent.	Area of Ferrite, sq. in.	Number of Ferrite Crystals.	Number of Ferrite Crystals per Square Inch.
38 *A*	850	¼	9.78	13.13	8.50	416	48.9
38 *B*	850	¼	7.21	13.13	6.27	531	84.7
39 *A*	825	¼	9.31	13.13	8.29	305	36.8
39 *B*	825	¼	8.50	13.13	7.38	612	82.8
47 *A*	725	¼	9.32	13.13	8.09	289	35.8
47 *B*	725	¼	8.73	13.13	7.58	440	58.0
48 *A*	700	¼	6.45	13.13	5.61	290	51.7
48 *B*	700	¼	9.05	13.13	7.86	665	84.5
54 *A*	675	½	6.55	13.13	5.68	212	37.3
54 *B*	675	½	6.72	13.13	5.83	242	41.5
55 *A*	675	8	8.30	13.13	7.21	403	56.0
55 *B*	675	8	7.17	13.13	6.23	350	56.2
62 *A*	650	25	9.21	13.13	8.00	367	45.8
62 *B*	650	25	9.40	13.13	8.18	389	47.6
61 *A*	650	23	6.58	13.13	5.72	294	51.4
61 *B*	650	23	6.44	13.13	5.59	288	51.5
67 *A*	625	75	8.81	13.13	7.65	378	49.4
67 *B*	625	75	7.60	13.13	6.61	406	61.8
64 *A*	625	70	7.83	13.13	6.81	597	87.6
64 *B*	625	70	7.43	13.13	6.46	564	87.4
70 *A*	600	220	8.13	13.13	7.06	335	47.5
70 *B*	600	220	8.47	13.13	7.36	417	56.6
68 *A*	600	210	8.05	13.13	6.99	313	44.8
68 *B*	600	210	8.57	13.13	7.44	335	45.0
74 *A*	550	1980	6.64	13.13	5.77	302	52.4
74 *B*	550	1980	7.16	13.13	6.22	426	68.5
73 *A*	550	1920	6.56	13.13	5.70	343	60.2
73 *B*	550	1920	6.18	13.13	5.37	321	59.8

NOTE.—*A* denotes section under zone of maximum depression; *B* denotes representative of a section that had undergone no crystal growth.

lying beyond the influence of the stress, implies recrystallization; no change implies at least little and probably no recrystallization. The values obtained for the two deciding heat treatments made at each temperature are given in Table I. The samples marked *A* are taken under the zone of maximum

depression, and those marked *B* at a section representing the average structure of the unstrained metal.

DETERMINATION OF THE TIME-TEMPERATURE RELATIONSHIP.

The authors started the work at 850° C. and worked at intervals of 25° down to 600° C., with an additional set of experiments at 550° C. These experiments seemed to uphold Chappell's[1] idea that the recrystallization process is an exceedingly rapid one, when once the suitable conditions are achieved. At the high temperatures, it was found that crystal growth would take place in a fraction of a minute.

The first test was run by putting into the furnace, which was maintained at a temperature of 850° C., a very small deformed sample and removing it the instant it reached the furnace temperature; the actual time that the specimen was in the furnace being but a fraction of a minute. Upon polishing and etching the sample, very noticeable crystal growth was in evidence. The photomicrographs shown in Figs. 4 and 5 are from the sample so treated.

Next the temperature of the furnace was lowered to 825° C. and a sample treated at that temperature in the same way as at 850° C. A result identical to that secured with the treatment at 850° C. was obtained. Similar conditions were found to exist with treatments run at 800, 775, 750, 725, and 700° C.

The temperature of the furnace was next lowered to 675° C., and it was found that a sample treated in the same way as those at the higher temperatures did not show crystal growth. A large number of experiments was run at this temperature, and in each case great care was taken to keep the temperature of the furnace constant and to accurately measure the time. It was found that heating the metal at 675° C. for seven minutes apparently did not affect its structure in the least, but that when the time was increased to nine minutes a very noticeable recrystallization resulted. This latter condition is shown in Figs. 6 and 7. Next a sample was held at 675° C. for exactly eight minutes and the results shown in Figs. 8 and 9 were obtained. A casual glance shows no difference in crystal size.

[1] "The Recrystallization of Deformed Iron," *Journal*, Iron and Steel Institute, 1914, No. 1, p. 460.

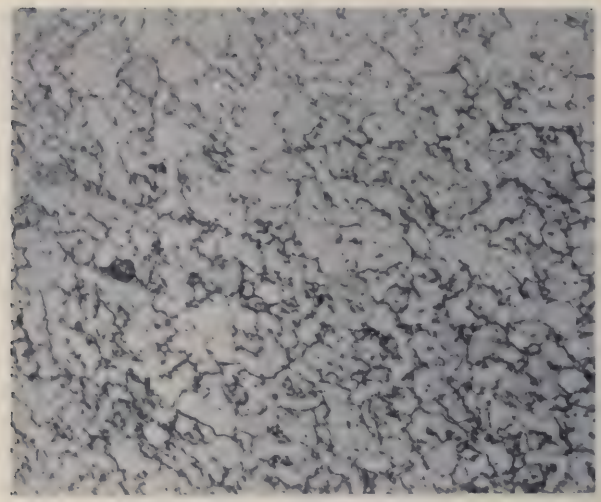


FIG. 4.—Sample Held at 850° C. for a Fraction of a Minute. View Taken under Depression, Showing Trace of Crystal Growth.

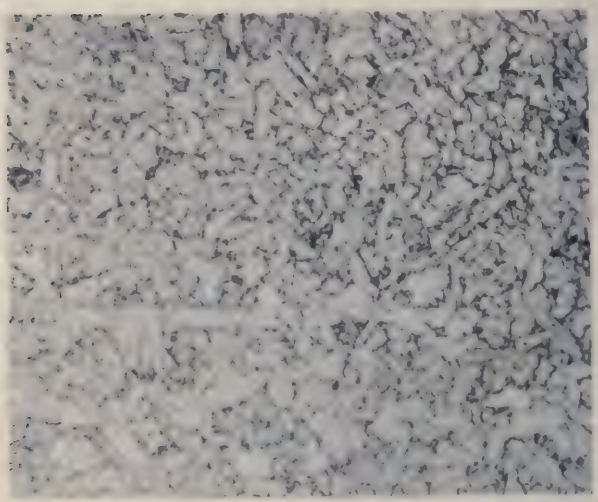


FIG. 5.—Same Sample as Fig. 4. View Taken at a Distance from Depression, Showing Original Structure.

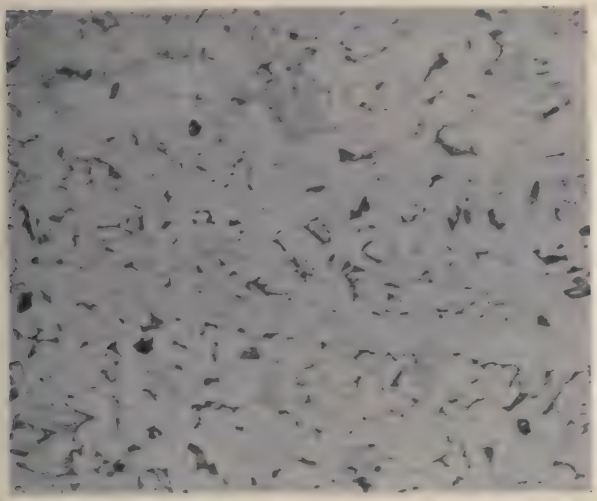


FIG. 6.—Sample Held at 675° C. for 9 Minutes. View Taken under Depression.

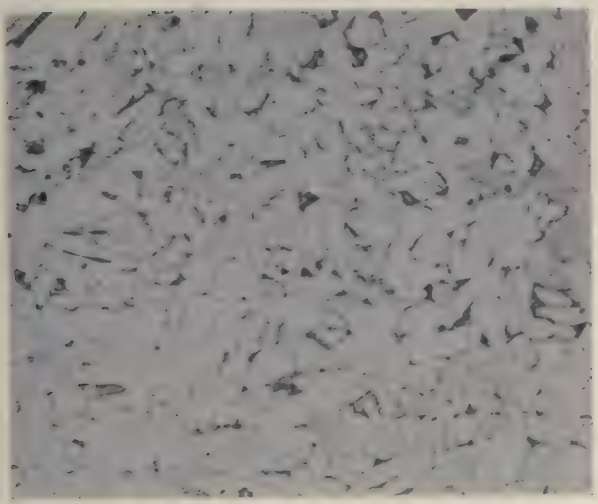


FIG. 7.—Same Sample as Fig. 6. View Taken at a Distance from Depression.

FIG. 8.—Sample Held at 675° C. for 8 Minutes. View Taken under Depression.

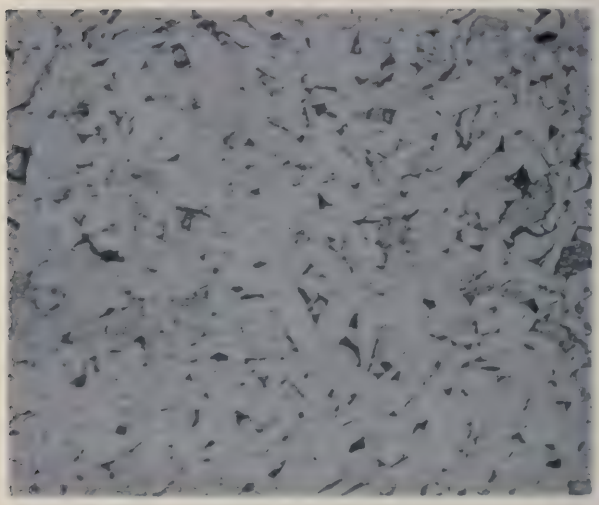


FIG. 9.—Same Sample as Fig. 8. View Taken at a Distance from Depression.

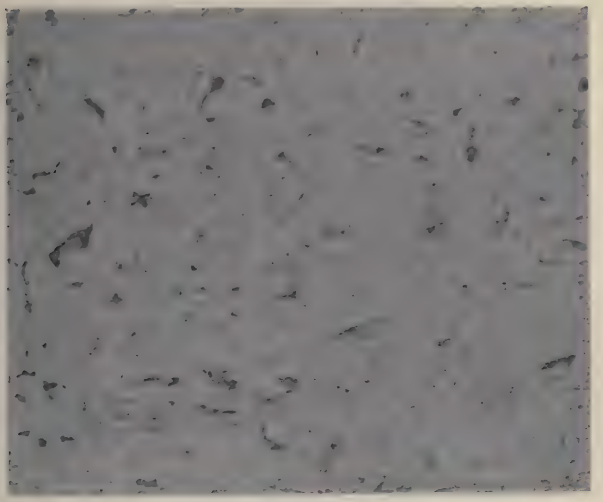


FIG. 10.—Held at 550° C. for 32 Hours. View Taken under Depression, Showing No Crystal Growth.

FIG. 11.—Same Sample as Fig. 10. View Taken at a Distance from Depression, Showing Original Structure.

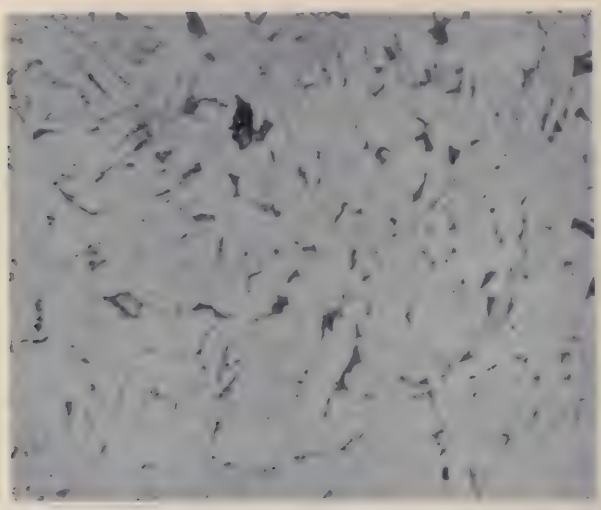


FIG. 12.—Held at 550° C. for 33 Hours. View Taken under Depression, Showing Slight Crystal Growth.

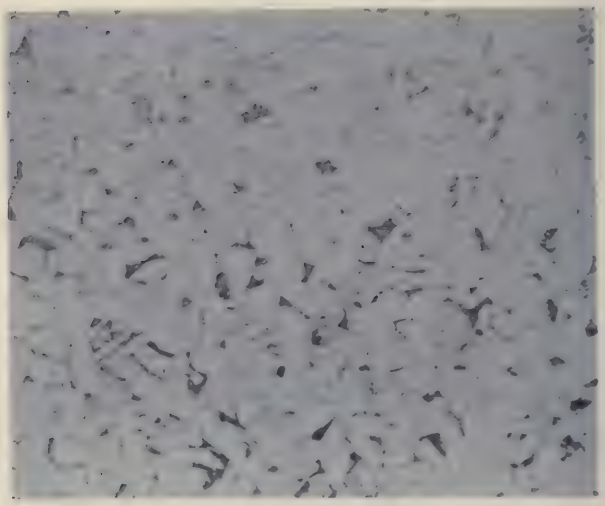


FIG. 13.—Same Sample as Fig. 12. View Taken at a Distance from Depression, Showing Original Structure.

The number of ferrite crystals shown in Fig. 8, which was taken from a section of the metal directly under the depression, is practically the same as in Fig. 9, which is representative of any section of the metal at a distance from the depression. The numerical values for these sections are given in Table I. If, however, the sample is subjected to macroscopic examination, as described earlier in this paper, a very slight change in the structure can be detected, although a suitable photograph could not be obtained to show this change distinctly. The whole experiment was repeated a number of times with different samples and identical results were obtained in each case.

The furnace was next maintained at a temperature of 650° C. and the time necessary for recrystallization to start under this new condition was investigated in exactly the same way as was adopted for the determination at 675° C. In this case the necessary time was found to be 24 minutes. At 625° C. it was found that a heating period of 1 hour and 10 minutes produced no crystal growth, but that heating for 1 hour and 15 minutes produced the anticipated change.

From a study of the time necessary for the development of crystal growth at the last three temperatures, a very interesting regularity was noticed. These time values were in a simple geometrical progression, having a constant equal to eight and a factor equal to three. This observation has led the authors, therefore, to advance the following hypothesis:

The time-temperature relationship affecting crystal growth on deformed low-carbon steel when heated at temperatures lying below the Ac1 critical point, and starting at 675° C., follows the laws of a simple geometrical progression for every fall of temperature of 25° C., that is, under these conditions, when T represents time in minutes, and a is equal to 8 and r equal to 3, the expression is

$$T = a,\ ar,\ ar^2,\ ar^3, \ldots\ldots\ldots ar^{n-1}$$

On the basis of this theory, a series of experiments was run to see if this simple progression held for lower temperatures. If the hypothesis were true, one should obtain crystal growth by heating deformed metal for 3 hours and 36 minutes at 600° C. Samples heated for 3 hours and 30 minutes showed no evidence

of crystal growth, although a treatment for 3 hours and 40 minutes gave good evidence of grain growth. As a further substantiation of the theory, the temperature was dropped to 550° C. and a heating for 32 hours and 25 minutes should have resulted in the production of crystal growth. A series of experiments was run for 32 hours and in no case was crystal growth evident.

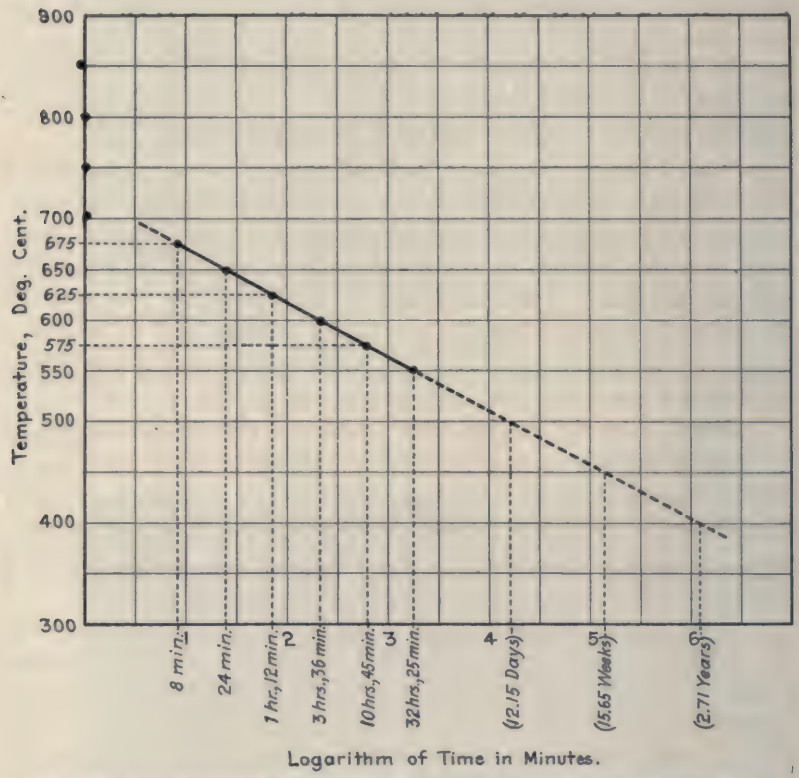


FIG. 14.—Relations of Time and Temperature to Crystal Growth in Low-Carbon Steel.

This is shown in Figs. 10 and 11. Another series of experiments was run for 33 hours, and in each case slight traces of recrystallization were evident. The changes in crystal sizes are shown in Figs. 12 and 13.

The slight variation of the temperature during the progress of the long heat treatments made it inadvisable to run experi-

ments for the exact time, as calculated by the hypothesis. Slight changes in the line voltage and variations in the temperature of the furnace room, were sufficient to offset the increase in accuracy obtained by so doing.

It is the opinion of the authors that complete equilibrium of a metal is never reached until it is composed of one crystal.

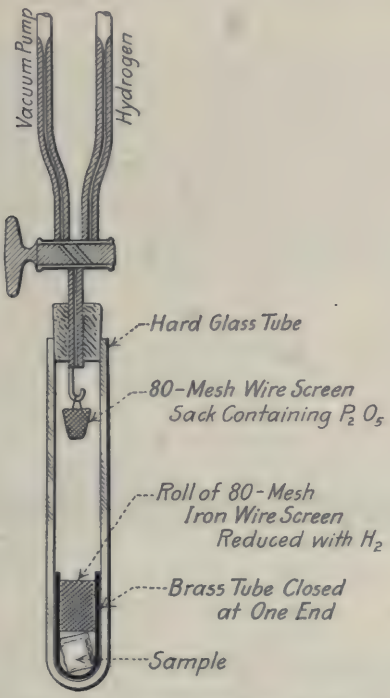


FIG. 15.—Apparatus Used in Heat-Treatment of Sample.

In other words, there exists in a metal at all times a tendency for the crystals to grow, and the finer the grain of the metal the greater is this tendency. If a metal be deformed, the crystals in the area of deformation are crushed and the equilibrium of the crystallin system is upset. Increases in temperature tend to give a higher velocity to this crystal growth. As previously stated, heating a deformed sample for nine minutes at 675° C. resulted in the production of crystal growth. By this is meant

that nine minutes was the time necessary to cause sufficient growth to be detected by the means employed.

It is unwise to positively assert that the recrystallization phenomenon on deformed low-carbon steel follows this simple progression at all lower temperatures, but the evidence at hand is sufficient, it is believed, to warrant one in advancing the above hypothesis, and in stating that in all probability such is the case.

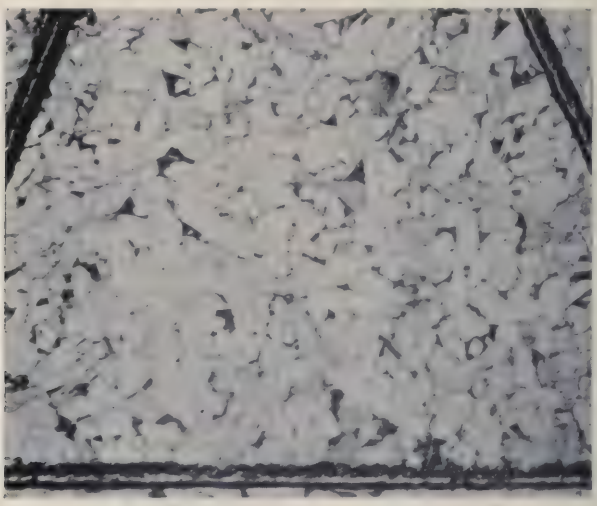


FIG. 16.—View under Depression, Showing Structure of Metal before Treatment.

A curve showing the relations just described, has been prepared from the evidence so far gathered, and is shown in Fig. 14.

THE ETCHED SURFACE OF THE DEFORMED METAL DOES NOT UNDERGO THE PROCESS OF RECRYSTALLIZATION.

At the beginning of the present investigation the authors held the opinion, that the ideal means for the detection of crystal growth would be to compare the structure of an etched-deformed sample at a section directly below the depression before heat treatment with the same section after the heat treatment. In attempting to develop growth by means of this supposedly ideal method, difficulty was encountered in obtain-

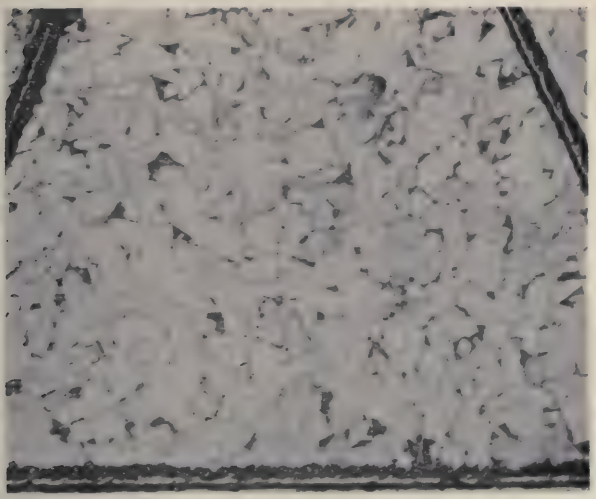


FIG. 17.—Same View as Fig. 16, after Sample was Held at 800° C. for 4 Hours.

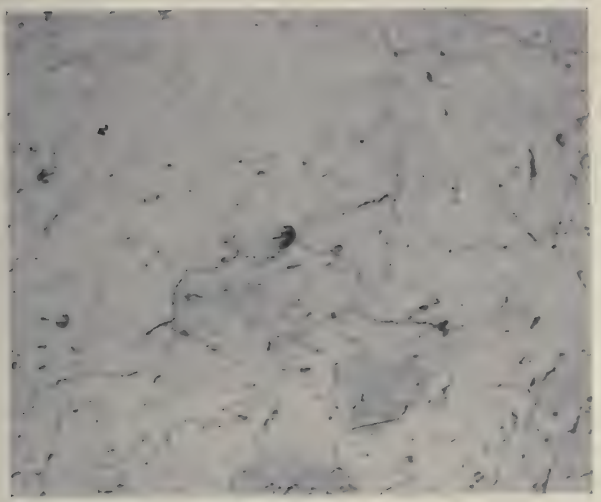


FIG. 18.—Same Sample as Fig. 17 after being Re-polished and Re-etched.

ing such an apparatus that the etched surface would suffer no oxidation during the heat treatment. After a long series of experiments the apparatus shown in Fig. 15 was adopted.

A very interesting result was obtained from the samples so treated. No change in the surface structure could be detected on metals annealed until after repolishing, when very coarse structures appeared just below the surface skin. A typical example of this is shown in Figs. 16, 17 and 18. Fig. 16 shows the structure taken from a section directly below the depression before treatment; Fig. 17 shows the same area after treatment and also exhibits the same structure; Fig. 18 shows the same area after repolishing and re-etching, and exhibits a very coarse structure. The surface shown in Fig. 18 was a small fraction of a millimeter below those shown in Figs. 16 and 17.

This result can be readily explained as follows: When a polished surface is etched, the intercrystallin material present is dissolved and the crystal boundaries therefore become distinctly manifest. The top of each surface crystal is independent, within all reasonable limits, of the top of every surrounding surface crystal, and each crystal therefore approaches a state of stable condition. Inasmuch as temperature changes, insufficient to produce a change of state, do not affect the stability of any independent crystal, the stability of all the surface crystals will remain the same throughout the heat treatment and no crystal growth will take place. If, however, the sample be repolished, a new surface is obtained a very slight distance below the upper one, and the above conditions then no longer hold good. The new surface crystals, being connected with each other through the medium of an intercrystallin material, are now no longer in a stable condition, and change in temperature during heat treatment may now cause crystal growth.

INDUSTRIAL APPLICATIONS.

One of the most important uses of low-carbon steel is in boiler plates and tubes. It was because of the failure of a number of boiler tubes used in one of the central heating stations in Detroit that the present investigation was undertaken. The tubes examined were taken from boilers operated at unusually high rates of evaporation for long periods of time. During the

cold months of the year, this condition was especially true, 250 per cent of rating being not uncommon. It was after considerable periods of such operation that a large percentage of the failures occurred. The rating of each of these boilers is about 725 horse power, and they furnish steam for heating purposes at a pressure of 125 lb.

Several tubes have been examined that had either failed or were removed because of bagging, and in every case crystal growth was evident; in fact several cases were found in which the metal exhibited an exceedingly coarse structure on the fire side of the tubes. Figs. 19, 20, 21 and 22 show the complete cross-section of one of the tubes examined; the order of the views shown being from the outside of the tube in. These photomicrographs show clearly that the outside of the tube must have reached a temperature high enough to have produced crystal growth.

Much work has been done by the Bureau of Mines[1] on heat transmission through boiler tubes. The experiments were carried out on a Heine water-tube boiler, operating under normal conditions with the tubes absolutely free from scale. They showed that the temperature of the tubes followed the temperature of the water and not of the products of combustion, and gave 200° C. as the temperature of the outside portion of the tubes.

In actual practice, however, it is the very rare exception to find boilers operated under such ideal conditions. Either they are run on overload or else the boiler feed water is of such a character as to produce a heavy deposit of scale. The second condition, or a combination of scale and heavy overloads, may cause serious difficulties in boiler operation.

The water fed to the boilers in question is considered to be much above the average. It carries about 125 parts per million of solids, of which between 6 and 9 parts per million are calcium sulfate ($CaSO_4$). Water with less than 200 parts per million of solids, of which 50 parts per million may be calcium sulfate ($CaSO_4$), is considered good; and by comparison it is evident that the boiler feed water in Detroit is well above the

[1] Kreisinger and Barkley, "Heat Transmission through Boiler Tubes," *Technical Paper No. 114*, Bureau of Mines, 1915.

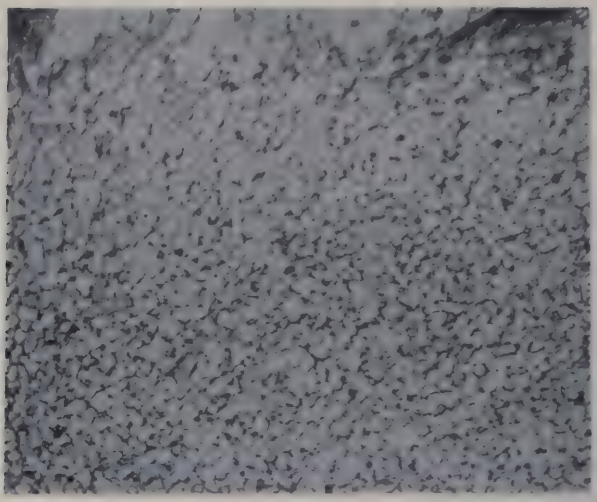


FIG. 19.—View of Outside Section of Boiler Tube, Showing Crystal Growth.

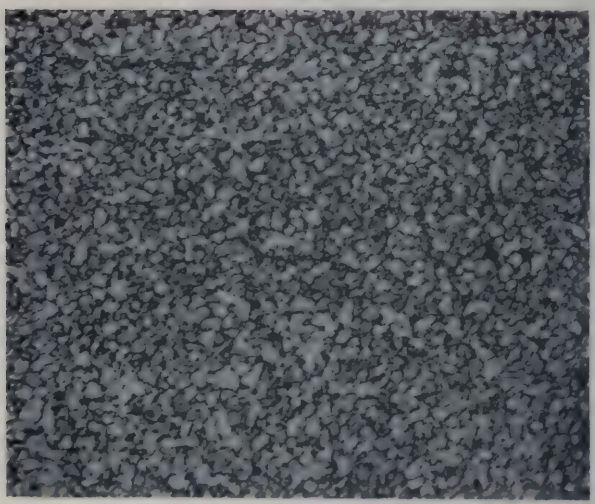


FIG. 20.—Continuation of Fig. 19 toward Inside of Tube.

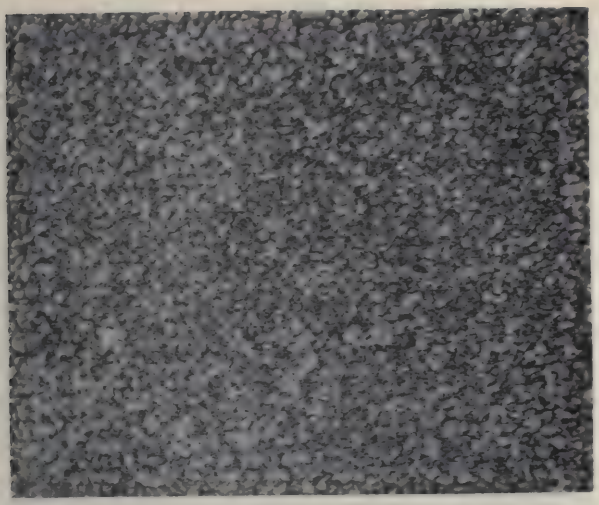


FIG. 21.—Continuation of Fig. 20 toward Inside of Tube.

FIG. 22.—Continuation of Fig. 21, Showing Structure of Inside Edge of Tube.

average. Furthermore, the absence of a large deposit of scale is a further substantiation of the excellence of the boiler feed water. Some idea of the small amount present may be obtained by an examination of a tube which had been in service for some time but which had been removed because it had developed a bag. A side view of the tube, giving an appreciation of the nature of the bag, is shown in Fig. 23, and an inside view, showing the depth of the scale, is shown in Fig. 24.

Under normal ratings, the bagging of a tube at this particular central station seldom occurs, but when high rates of evaporation are maintained over long periods of time, trouble of a more or less aggravating nature is encountered. It is felt that the difficulty is due to heavy ratings combined with thin scale deposits. On this hypothesis, the feed water of these boilers

FIG. 23.—Side View of Boiler Tube, Showing Nature of Bag.

this past winter has been treated with soda ash.[1] This has decreased the quantity of scale and has lessened, although it has not completely eliminated, the trouble. A still further attempt to lessen the quantity of the scale without actual resort to filtration of the boiler feed water is about to be made, and it is trusted that this last step will eliminate in large measure the troubles due to bagging.

It is appreciated that there are a number of people who feel that too high an overload, alone, will produce bagging. No reliable information to substantiate such a belief has been brought to the attention of the writers, and, therefore, until it becomes evident that scale is not responsible for this particular trouble, such a hypothesis will not be tenanted.

The authors feel that the presence of a thin film of scale

[1] Badger, "An Unusual Case of Water Purification," *The Michigan Technic*, Vol. 29, No. 1 1916, p. 10.

on the inside of a tube may so interfere with a rapid heat transmission that a tube may become locally overheated. The temperature to which the metal may rise will be well above that found in tubes free from scale, and yet below the upper critical temperature of the metal. Such a temperature would in no way affect grain size in thoroughly annealed metal, but it may materially induce and produce grain growth in low-carbon steel previously deformed at temperatures below its critical point.

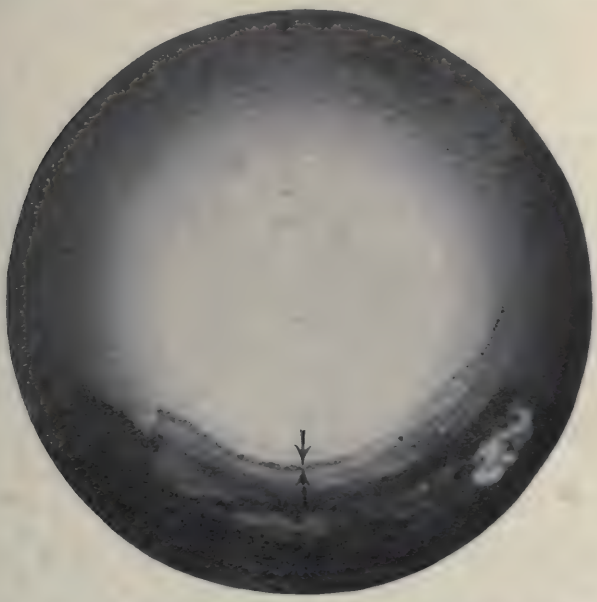


FIG. 24.—Inside View of Same Tube, Showing Depth of Scale.

This latter phase is the condition almost universally encountered in the boiler tubes used in present-day practice. Under the best of refinements it is difficult to see how a tube perfectly free from strains can be obtained. Therefore, it is difficult to see how a tendency for coarse crystallization from such metal can be prevented. It is possible to make tubes with a higher carbon content, or possibly with the same carbon content as at present but containing a special element, such as nickel, which will not possess the same strong recrystallization tendency.

These features merit consideration, and in fact, the first one is about to undergo a trial at one of the central heating stations in Detroit.

The extent to which coarse grains make low-carbon-steel boiler tubes weak and brittle, however, was determined by a number of tension and fatigue tests. Three sets of samples were employed; the first of untreated metal, the second of metal annealed for ten minutes at 950° C. and then cooled in the furnace, and the third of coarse-grain metal, produced by stressing all of the test specimens the same amount and in all

TABLE II.—TENSION TESTS.

Sample No.	Treatment.	Elastic Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Ultimate Strength, lb. per sq. in.	Elongation in 6 in., per cent.	Reduction of Area, per cent.
1	As received...	33 190	55 250	42 300	13.30	56.1
2	As received...	35 650	53 400	41 500	14.50	55.2
3	As received...	34 410	54 700	42 500	15.30	54.8
Average.	.....	34 420	54 450	42 100	14.37	55.4
4	Annealed.....	22 550	51 620	36 250	21.66	54.5
5	Annealed.....	22 100	50 000	38 150	18.32	54.4
6	Annealed.....	20 450	49 900	36 180	21.35	54.6
Average.	.....	21 700	50 507	36 860	20.44	54.5
7	Coarse-grain..	14 700	39 700	31 750	17.15	54.7
8	Coarse-grain..	12 850	39 650	27 900	19.50	55.4
9	Coarse-grain..	14 800	39 560	32 100	19.68	55.7
Average.	.....	14 117	39 633	30 583	18.78	55.3

cases past the elastic limit, and following this by an annealing for a period of three hours at a temperature of 800° C.

The elastic limits and tensile strengths of the untreated samples averaged 34,420 lb. per sq. in. for the former and 54,450 lb. per sq. in. for the latter; while the values for the annealed samples were 21,700 lb. per sq. in. and 50,507 lb. per sq. in., respectively. Such results showed that these samples had as good physical properties as could be expected from any metal of like composition and heat treatment. The elastic limit and the tensile strength of the coarse-grain samples averaged 14,117 and 39,633 lb. per sq. in., respectively. These results were 58.2 and 43.1 per cent lower than were the results of the untreated samples, and proclaimed, therefore, a decidedly

inferior grade of metal. The complete figures for these tests are given in Table II.

The actual stresses occurring in a boiler tube are tension and shear. If the principal stress resulting from a combination of the tension and shear becomes greater than the elastic limit of the metal, failure will take place. Since a coarse-grain metal has such an exceedingly low elastic limit, its use should be avoided when it is possible to get metal with a much higher elastic limit.

The tension tests noted above were carried out at room temperature. Rosenhain and Humfrey[1] have conclusively demonstrated that temperature has an important influence on the physical properties of steel. If actual boiler conditions were duplicated, employing as a necessary result higher temperatures during the test proper, it is felt that still more striking differences would have been brought forth. These authors have shown that the more amorphous material present in a metal, the greater is its tensile strength. They have also shown that the amorphous material has a greater tensile strength at ordinary temperature than the crystallin material, but that as temperature rises, producing conditions conducive to a greater ease of flow, its tensile strength decreases. At about 800° C. the tensile strength of such a substance becomes even lower than that of the crystallin material. Because of this property of the amorphous intercrystallin material, the elastic limit of a boiler tube is most certainly lower when in actual service than when at atmospheric temperature. This condition, furthermore, is greatly intensified when a layer of scale causes the temperature of the tube to rise unduly.

A very interesting development in connection with the tension tests is the fact that the percentage of elongation and the percentage of reduction of area remain practically constant throughout the entire series. This shows that the ductility of the metal is but little affected by the size of the crystals. It may therefore be said that coarse-grain ferrite is weak, but not brittle.

In Table III are given the results of several fatigue tests run

[1] "The Tenacity, Deformation, and Fracture of Soft Steel at High Temperatures," *Journal*, Iron and Steel Institute, 93, No. 1, p. 219.

on samples prepared in the same way as those for the tension tests. The coarse-grain metal was found to have an average resistance to fatigue 16.43 per cent lower than the untreated and 77.75 per cent lower than the annealed samples. This is very important when one considers that boiler tubes are constantly under the influence of alternating stresses.

TABLE III.—FATIGUE TESTS.

Treatment.	Sample No.	Cycles for Fracture.
As received	1	405
	2	420
	3	475
	4	445
	5	458
	6	430
	7	464
	8	485
	9	496
	10	479
	Average	457
Annealed	11	705
	12	830
	13	756
	14	706
	15	726
	16	792
	17	725
	18	722
	19	737
	20	751
	Average	745
Coarse grain	21	419
	22	345
	23	375
	24	392
	25	368
	26	415
	27	405
	28	355
	29	397
	30	351
	Average	382

NOTE.—Dimensions of all specimens: width, 1 in.; thickness, 0.127 in.
Throw of machine, 2 in.
Speed of machine, 267 r.p.m.

CONCLUSIONS.

The many conditions studied and developed have resulted in the acquisition of a number of generalizations which, it is trusted, will prove to be of some value in certain connections. The six most important ones are summarized as follows:

1. The time-temperature relations for every fall of tempera-

ture of 25° C., starting at 675° C., governing the recrystallization of deformed low-carbon steel at temperatures below the Ac1 critical point, follow the formula for geometrical progression, that is,

$$T = a,\ ar,\ ar^2,\ ar^3, \ldots\ldots\ldots ar^{n-1},$$

in which T represents time in minutes and a and r are equal to 8 and 3, respectively.

2. The etched surface of deformed low-carbon steel does not undergo the process or recrystallization when heated, even for long periods of time at temperatures below the critical point, although the metal immediately below the surface does show such change.

3. The recrystallization process on deformed low-carbon steel is an exceedingly rapid one at all temperatures below the A3 point and above 700° C. and between these limits recrystallization takes place in all cases in a fraction of a minute.

4. It is felt that perhaps the transformation occurring at 690° C. has some effect upon the recrystallization process, since crystal growth took place at 700° C. in a fraction of a minute and at 675° C. in eight minutes. The present investigation has been insufficient, however, to warrant any definite statement on the matter. It certainly offers a splendid opportunity for further work.

5. The evidence obtained during the present investigation seems to show that boiler tubes, if kept free from scale, will never fail because of the recrystallization of the ferrite crystals. A very thin layer of scale in the tubes, however, resulting in rise in temperature of the metal, may result in the recrystallization phenomenon.

6. Low-carbon steel in a deformed state is so stable at atmospheric temperatures that the time-temperature influence alone, at such temperatures, would probably require thousands of years for the production of crystal growth.

BIBLIOGRAPHY.

Badger, "An Unusual Case of Water Purification," *The Michigan Technic*, Vol. 29, No. 1, 1916, p. 10.

Chappell, "The Recrystallization of Deformed Iron," *Journal*, Iron and Steel Institute, 1914, No. 1, p. 460.

Charpy, "Sur la maladie de l'écrouissage," *Revue de Métallurgie*, Memoirs, Vol. 7, 1910, p. 655.

Goerens, "On the Influence of Cold-Working and Annealing on the Properties of Iron and Steel," *Journal*, Iron and Steel Institute, Carnegie Scholarship Memoirs, Vol. 3, 1911, p. 320.

Kreisinger and Barkley, "Heat Transmission through Boiler Tubes," *Technical Paper No. 114*, Bureau of Mines, 1915.

Le Chatelier, "Notes de Métallographie," *Revue de Métallurgie*, Memoirs, Vol. 8, 1911, p. 367.

Longmuir, "Some Aspects of Wire Drawing," *Journal*, Iron and Steel Institute, 1912, No. 2, p. 142.

Robin, "Recherches sur le développement des grains des metaux par recuit après écrouissage," *Revue de Métallurgie*, Memoirs, Vol. 10, 1913, p. 752.

Rosenhain and Humfrey, "The Tenacity, Deformation, and Fracture of Soft Steel at High Temperatures," *Journal*, Iron and Steel Institute, 1913, No. 1, p. 219.

Sauveur, "Notes on the Crystallin Growth of Ferrite below its Thermal Critical Range," *Proceedings*, International Association for Testing Materials, Sixth Congress, 1912, Vol. 2.

Stead, "The Crystallin Structure of Iron and Steel," *Journal*, Iron and Steel Institute, 1898, No. 1, p. 145.

DISCUSSION.

THE CHAIRMAN (PAST-PRESIDENT HENRY M. HOWE).— Gentlemen, this is a very important contribution to our knowledge of an important and extremely difficult and complicated subject. I should like to call your attention to some important articles which have lately appeared, one by Fahrenwald and one by Mathewson and Phillips, in the Bulletin of the Institute of Mining Engineers for January, 1916 (pages 1 and 103), and a very modest but extremely brilliant one by Professor Jeffries, of the Case School of Applied Science, in the May Bulletin of the Institute, page 987. Professor Jeffries brings out there the remarkable fact that for every different degree of deformation there is a specific temperature of rapid grain growth, the temperature being the lower the greater has been the strain. It was shown sixteen years ago by Muir[1] that the temperature at which the softening of cold-worked iron begins is the lower the greater the deformation has been, but this has been worked out by Jeffries in a way which explains the extraordinary phenomenon discovered by our fellow member, Professor Sauveur, of what he called the critical strain, which would produce coarsening in carbon steel. It appears from Jeffries that this must be re-stated, that there is a critical relation between strain and temperature, that for every temperature there is a critical strain and for every strain there is a critical temperature. **The Chairman.**

MR. C. F. HIRSHFELD.—Until quite recently, boilers were hand-fired or they were fired by stokers burning approximately the same amount of coal per unit of time and area. Within the past few years some of the large central stations have, however, adopted stokers which can be made to burn coal at much greater rates and have modified their practice accordingly. In such plants it is now customary to carry the peak loads on boiler equipment previously limited to outputs of one-half or even one-third of the quantities now demanded. **Mr. Hirshfeld.**

[1] "On the Tempering of Iron Hardened by Overstrain," *Philosophical Transactions*, Royal Soc., read Dec. 6, 1900, Vol. 198, Jan. 23, 1902.

Mr. Hirshfeld. This simply means that the metal walls of the tubes are called upon to transmit two to three times the quantity of heat previously transmitted in a given time.

Experience and experiment have led boiler makers and boiler users to the conclusion that, if the water surface of a tube can be kept free from scale, it can safely transmit heat at even greater rates than are now demanded. Unfortunately, it is commercially impracticable to keep tubes free from scale, even with distilled make-up water, and the operation of boilers at high ratings is therefore very apt to make it necessary to devise means of operating such tubes safely even though they be moderately scaled.

Under the circumstances we are naturally interested in studying possible solutions. One of the most interesting possibilities is given in the summary of this paper, namely, the use of steels with higher carbon content than is now customary. We are going to try that experiment in some of the plants of the company with which I am connected.

The United States Navy has been driven to the use of nickel-steel boiler tubes by somewhat similar conditions, and, so far as we have been able to discover, have obtained satisfactory results.

In closing I should like to lay stress upon a certain fact which was mentioned by the chairman in the discussion of an earlier paper. Many of our specifications are built on little better than superstition. We have found by experience that certain combinations worked well under certain conditions and we have been loath to adopt new combinations even after the conditions have been greatly modified or even absolutely changed. Our present specifications for boiler materials are based upon experience under conditions quite different from those existing in many of the central stations at the present time, and it is not at all inconceivable that much better results may be obtained with material purchased on very different specifications.

Mr. Speller. MR. F. N. SPELLER.—The paper just presented is interesting, especially the portion dealing with the time-temperature factor in recrystallization of steel. However, I would differ somewhat with the author's conclusion as to the difference in

physical properties of annealed and recrystallized steel. The authors state in their paper: "The extent to which coarse grains make low-carbon-steel boiler tubes weak and brittle, however, was determined by a number of tension and fatigue tests." These test specimens were recrystallized throughout the entire section, but unfortunately the authors show no microphotographs or other evidence to indicate the extent of the recrystallization or whether the structure of these samples is at all comparable with that noticed on the surface

Mr. Speller.

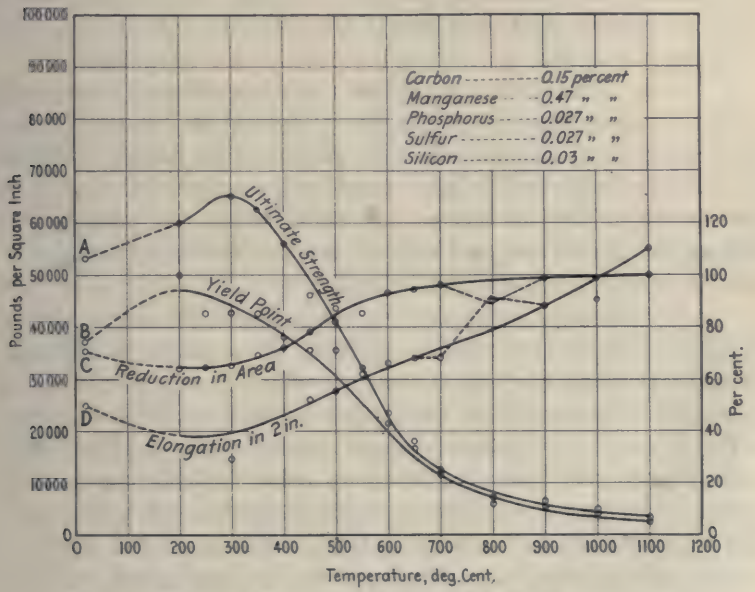


FIG. 1.—Physical Properties of Steel at High Temperatures.

of the bagged tubes. Microphotographs of the latter indicate that the recrystallization is confined to the surface of the tube and only extends to a depth of about one-tenth of the thickness of the metal, so that, assuming that the crystallin portion was 30 per cent lower in tensile strength, as indicated in the recrystallized test specimens which the authors tested, this would mean only 3 per cent less strength in the wall of the tube, which would of course be of no consequence.

The bagging of boiler tubes in service is an old experience, occurring every day under certain conditions of service due to

Mr. Speller. the insulation of the inside surface from the water and the overheating of the metal underneath the scale. Some physical tests at various temperatures were made at one of our laboratories on seamless tube steel of the same grade as the authors used in their experiments, the results of which are shown in Fig. 1. It will be noticed that the tensile strength drops very rapidly as the temperature rises above 300° C., until at 700° C. the strength is about 12,000 lb. per sq. in. If there was enough scale on the tubes to cause the metal on the fire side to be heated to this extent a little spurt in the firing might very readily raise the temperature sufficiently to cause the tube to fail from bagging.

The new A. S. M. E. Boiler Code specification for boiler tubes requires that the tubes be up to the thickness specified at all points and specifies an extra thickness for higher pressures. This should help to minimize trouble due to overheating of the tubes, but so long as boilers are forced to 250 per cent of their rating, which it is said was not uncommon in this particular case, trouble of this kind may be expected.

As to the suggestion that higher carbon steel might be used in tubes, this would probably increase the strength of the tubes in approximately the same proportion at all temperatures, but it should be remembered that the tube will be stiffer and more difficult to set and probably more difficult to keep tight. This can only be determined, however, by actual experience, and I am glad to note that a trial is being made along this line. Obviously, the surest means for remedying these conditions is to use more boilers and operate them nearer to the normal rating.

Mr. Hirshfeld. MR. HIRSHFELD.—In my previous remarks I called attention to the higher ratings at which modern boilers are operated. The choice of the proper number of boilers to install is entirely an economic problem and it is practically capable of an exact solution.

In those remarks I failed to call attention to one thing which serves very well to give a conception of the meaning of these high ratings. If you will examine Fig. 23 of the paper, you will note that the outside of the tube has a mottled appearance; it looks as though something had been plastered over it. This material is fused ash which was carried up by the furnace gases and deposited on the tube in a molten condition. Such

deposition of ash occurs over the entire length of the tubes in some of our boilers, even though the tops of these tubes are about 33 ft. above the fuel bed. **Mr. Hirshfeld.**

MR. A. E. WHITE.—In answer to Mr. Speller's query relative to the degree of coarse crystallization in the "coarse-grain" samples subjected to the physical tests, it is advised that the size of the crystals is about equal to those shown in Fig. 18. **Mr. White.**

I am very sorry that the photomicrograph in Fig. 19 does not bring out better the depth of the coarse crystallization. From an actual measurement, however, we found that the extent of this particular coarse crystallization, starting at the fire side, was about one-sixteenth of the thickness of the tube.

We have found on all bagged tubes that either the metal was coarsely crystallin on the fire side or else there were blow-holes and cavities near the water side. Therefore, we have felt that bagging, in general, is either due to an over-segregation of cavities and blowholes—and that is a point which merits consideration, but which we have not touched on in this paper—or to coarse crystallization.

The question as to whether or not it is possible to keep tubes in place in the tube sheet easier with a higher carbon content than with a lower carbon content is one which was very carefully considered before suggested changes were made. It is felt that a higher carbon tube, when once rolled into the tube sheet, will remain in place better than the more ductile lower carbon tube. It was anticipated that the boiler makers might object to rolling the higher carbon tubes into the tube sheet, but quite a number of such tubes have already been placed in boilers with no apparent difficulty.

THE CHAIRMAN.—This grain coarsening is not a self-arresting process, but a self-continuing one; that is to say, a coarse grain is more stable than a fine one, and the coarse grains will continually feed on the finer ones with which they are in contact until they absorb them and in time all become one grain.[1] **The Chairman.**

[1] Since the meeting I have met evidence tending strongly to show that, at least under certain conditions, the grain growth of some metals does arrest itself on reaching a certain grain size varying with the conditions. It seems doubtful to me whether this is true of iron, at least within any moderate limits, because of the enormous size which the grains of meteorites reach. These are reported as often consisting of a single grain, suggesting that here almost infinite time at the grain-growth temperature leads to almost infinite growth.

The Chairman. In regard to the higher carbon content, I doubt if it is necessary to go very far in order to get what we need. We are all thankful for the wonderful properties of the metal, iron, and here we have a new cause for thankfulness. Most of the other metals on which we operate are homogeneous: bronzes, brasses, copper, etc., are homogeneous and grain growth, once started, can keep on indefinitely, so far as we now know. In the case of iron, the presence of pearlite may act as an absolute bar to grain growth; I do not say it will, but it is instructive that Professor Sauveur, on my failing to get this growth on various samples of steel, admitted that he had found it was only in a very narrow range of carbon content that he got coarsening. With over 0.12 per cent of carbon, he has never been able to get any coarsening. That percentage of carbon implies the presence of 13 per cent of pearlite; and this amount of pearlite scattered through the material may well envelope the several grains of ferrite so completely as to act as a bar to any grain growth whatever. I do not know whether Mr. Speller would find that too hard to weld or not, without too much increase in cost.

(*Addendum by letter*).—If the temperature rises as high as 600° C. this pearlite is likely to split up into its constituents, of which the ferrite joins that which constitutes the great mass of the metal, while only 1.8 per cent of cementite would remain in a steel of 0.12 per cent of carbon, to act as a parting between the ferrite grains. Clearly a much larger carbon content would be needed at such temperatures to prevent grain growth.

Mr. Speller. MR. SPELLER.—The analysis of the tubes with which the authors experimented showed 0.113 per cent carbon, which is a little low for standard steel seamless boiler tubing, which ranges between 0.12 and 0.18 per cent carbon.

ALUMINUM BRONZE: SOME RECENT TESTS AND THEIR SIGNIFICANCE.

By W. M. Corse and G. F. Comstock.

SUMMARY.

In this paper are presented results of tension tests and endurance tests with the Landgraf-Turner and White-Souther machines, made on manganese bronze and aluminum bronze, showing that although the former alloy may give better figures when tested in tension, the latter is far superior in endurance of alternating stresses. A few alternating-stress tests on phosphor-bronze, malleable cast iron, and rail steel are cited in comparison. The significance of the alternating-stress test is explained, and the proper method of reporting results by curves is described by quoting from Rosenhain's book on Physical Metallurgy. Carpenter and Edwards' Eighth Report to the Alloys Research Committee of the British Institution of Mechanical Engineers is also quoted as checking and explaining the combination, in aluminum bronze, of low yield point in tension with great endurance of alternating stresses.

A method of heat treatment is explained and described in detail, by means of which the proportional limit of 10-per-cent aluminum bronze was raised very substantially without the loss of too much ductility. It is shown that this method is applicable to castings of ordinary size, as well as to small test specimens, and that it increases the endurance under alternating stresses as well as the proportional limit in tension.

ALUMINUM BRONZE: SOME RECENT TESTS AND THEIR SIGNIFICANCE.

By W. M. Corse and G. F. Comstock.

In the course of our work in making high-quality castings numerous tension tests on 10-per-cent aluminum bronze were made, mostly on small cast-to-size bars made in green-sand molds. Average results of these tests gave close to the following figures:

Proportional limit, lb. per sq. in	19 800
Yield point, lb. per sq. in	24 000
Tensile strength, lb. per sq. in	77 000
Elongation in 2 in., per cent	24.5
Reduction of area, per cent	25.2

The proportional limit was determined with the autographic device provided on the Olsen universal testing machines, and it is accurate to about 0.003 in. The yield point in this laboratory is taken, for non-ferrous metals, as the stress producing a deformation of 0.5 per cent, or 0.01 in. in the usual gage length of 2 in. It will be readily seen that the above figures for proportional limit and yield point are low, even for bronze, and manganese bronze may rightly be claimed to show superior figures for the same properties.

Many engineers, basing their design of machine parts or structural members on the elastic limit of the material used, in preference to its ultimate strength, might avoid aluminum bronze and give preference to manganese bronze on account of the better showing of the latter in a tension test. But in the great majority of cases, materials used in engineering construction are not subjected to uniform or slowly changing stresses as in a tension testing machine, but to alternating stresses, more or less suddenly applied, and varying in direction and intensity. The important point for an engineer to know, in most cases, is the life which a material will show under conditions of alternating stresses of small intensity, rather than the greatest possible

load that may be applied without permanent deformation. As a matter of fact, machines and structures are never designed so that any of their parts are stressed up to their elastic limits, because it is known that such stresses repeated in practical use would cause very rapid failure in steel, ordinary bronze, etc.

ALTERNATING AND REPEATED STRESS TESTS.

Tests with Landgraf-Turner Machine.—To give more certain indications than are afforded by a tension test in regard to the life of a material under alternating and repeated stresses, many different types of endurance or fatigue testing machines have been used. In the opinion of many, a machine of this type, in order to be practical, must give quick results—that is, results obtainable in a few minutes, instead of a few days or weeks. A machine intended to satisfy this demand is the Landgraf-Turner alternating impact machine, which was favorably described in a paper by J. B. Kommers.[1] The test specimen for this machine is a cylindrical rod $\frac{3}{8}$ in. in diameter and 8 in. long. One end of the specimen is held rigidly during the test, while the other is bent backward and forward through a small angle by blows repeated 400 times per minute. The number of alternations endured before fracture is recorded automatically. Since fracture takes place close to the stationary end of the specimen, each end may be tested, and thus two tests obtained from each specimen. Some tests were made here with this machine on three different types of bronzes and the results may be of sufficient interest to warrant reporting them in this paper.

Bars of our alloys Nos. 3, 5 and 29, $\frac{5}{8}$ in. in diameter, were cast in sand for these tests and were machined accurately to a diameter of $\frac{3}{8}$ in. Alloy No. 3 is a phosphor-bronze containing 11 per cent of tin; No. 5 is a regular 10-per-cent aluminum bronze with 1 per cent of iron; and No. 29 is best-quality manganese bronze, really a brass with 41 per cent of zinc, 1 per cent each of iron and tin, and about 0.5 per cent each of manganese and aluminum. As a preliminary trial one bar of each alloy was tested in the machine as used for steel, with a $\frac{3}{4}$-in. slot between the hammers that act on the test specimen.

[1] "Repeated Stress Testing," *Proceedings*, Int. Assn. Test. Mats., Sixth Congress, Section V, Paper 4a, (1912).

The results obtained, each the average of two tests, were as follows:

	ALTERNATIONS.
Alloy No. 3	75
Alloy No. 5	4485
Alloy No. 29	165

Such low results for alloy No. 3 were shown that it was thought best, for the sake of accuracy, to test this alloy with the machine adjusted as for malleable cast iron, with a 1-in. slot between the hammers. This makes the test less severe, as the angle through which the bar is bent at each alternation is considerably less than with the $\frac{3}{4}$-in. slot. The results obtained in this way from nine bars that showed clean sound metal in

TABLE I.—RESULTS OF LANDGRAF-TURNER ENDURANCE TESTS, WITH 1-IN. SLOT BETWEEN HAMMERS.

Material.	Number of Alternations to Produce Failure.		
	Maximum.	Minimum.	Average.
Alloy No. 3	2460	600	1437
Malleable cast iron	150	60	83

their fractures, together with those obtained on an equal number of bars of malleable cast iron, tested under exactly similar conditions, are given in Table I.

The specimens of alloys Nos. 5 and 29 were tested with a $\frac{3}{4}$-in slot in the machine, and these results may therefore not be compared directly with those obtained for alloy No. 3. But as the $\frac{3}{4}$-in. slot is used for tests of steel, the results given in Table II are properly comparable to those obtained from steel axles and rails, which run from about 700 to 1700 alternations. The highest number of alternations ever obtained from a steel in this laboratory was 1715, from the head of an open-hearth rail. In making up this table, the results obtained from specimens showing good fractures were separated from those where the fractures indicated defective metal, because some of the specimens of alloy No. 29 were defective.

In considering the results, those from the good fractures of alloy No. 29 should be taken as representing this alloy, and they are seen to be within the range of ordinary steel. The results given by aluminum bronze (alloy No. 5) are far superior to steel, however, and show that this alloy has a truly remarkable endurance of severe alternating stresses. Professor Arnold of Sheffield, England, found the same result in testing aluminum bronzes with a machine very similar to the Landgraf-Turner type, and reported to the authors of the Eighth Report to the Alloys Research Committee of the British Institution of Mechanical Engineers that while the 9.9-per-cent aluminum bronze gave very good results, the 7.35-per-cent alloy "constituted a record in its capacity of resisting alternations."

TABLE II.—RESULTS OF LANDGRAF-TURNER ENDURANCE TESTS, WITH $\frac{3}{4}$-IN. SLOT BETWEEN HAMMERS.

Material.		Number of Alternations to Produce Failure.		
		Maximum.	Minimum.	Average.
Alloy No. 5	10 good fractures.....	7230	3690	5457
	10 bad fractures.....	5910	3540	4983
	All specimens.....	....	....	5220
Alloy No. 29	3 good fractures.....	1050	750	910
	15 bad fractures.....	1200	30	278
	All specimens.....	....	....	383

The question will probably arise as to why further tests were not made with this machine, especially on manganese bronze, in view of the large number of defective specimens, and the answer to this inquiry would be that since a better, more trustworthy method of testing the endurance of these alloys was available, it did not seem worth while to repeat the less valuable tests. In the Landgraf-Turner machine, alternating stresses of high intensity are used exclusively, to shorten the time of the tests, and at every blow of the machine the elastic limit of the test specimen is exceeded and a permanent set is produced. These conditions are of course much more severe than are met with in ordinary practical use of any metal. In order to show the probable life of a material in practical use, the test made on it should duplicate the conditions of such use as closely as

possible, or in other words, the alternating stresses should be of low intensity, and preferably below the elastic limit of the material.

Tests with White-Souther Machine.—Tests under these conditions have been made with our White-Souther machines, which operate on an entirely different principle from the Landgraf-Turner type. The White-Souther machine rigidly holds the test specimen which is machined accurately to a special shape, in the axis of a wheel which is revolved at a speed of 1300 r. p. m. The ends of the test specimen projecting on each side of the wheel are loaded with weights which hang constantly downward, so that each complete revolution of the wheel involves two reversals of stress in the specimen. This stress varies at any given instant of the test from a maximum value in compression at the under side of the specimen, to zero at the central axis, and then to a maximum value in tension at the upper side. The maximum value of the fiber stress may be easily computed from the known load applied at the end of the specimen, since the free length and diameter of the end of the specimen, which is the tested portion, are always made exactly the same, and can of course be accurately measured. A counter at each end of the machine records automatically the number of hundred revolutions endured before fracture, and the machine is allowed to run continuously day and night until both ends of the bar are broken.

This test is essentially identical with the well-known Wöhler endurance test, and a very clear exposition of its practical meaning is given by Dr. Walter Rosenhain in his recent book on physical metallurgy.[1] It is explained that a single endurance test made on each of two metals is not a good basis for judgment as to their behavior in actual service, for their comparative endurances at other stresses than that used in the test may be entirely different. For the same reason, alternating-stress tests made with the Landgraf-Turner or Upton-Lewis machines, where high stresses are used in order to shorten the time consumed in testing, are of small value even if a series of tests is made, for they do not indicate the endurance under stresses of low intensity. To determine the true safe range of stress, or the stress which a given material will resist indefinitely, it is necessary, according to Rosenhain, "to carry out a Wöhler

[1] "An Introduction to the Study of Physical Metallurgy," pp. 224–230.

or similar alternating-stress test under at least three, but preferably four, different intensities of loading, so chosen that under the heaviest loading the test-bar breaks after a few thousand reversals, the subsequent loadings being diminished in such a way that under the last—and lightest—loading the bar either does not break at all or only does so after several million reversals. The results thus obtained are then plotted with the number of reversals endured as abscissas and the intensity of the stress as ordinates. It is found that the points so plotted lie on a curve of parabolic form which tends to become parallel to the axis as the intensity of the stress diminishes. The horizontal

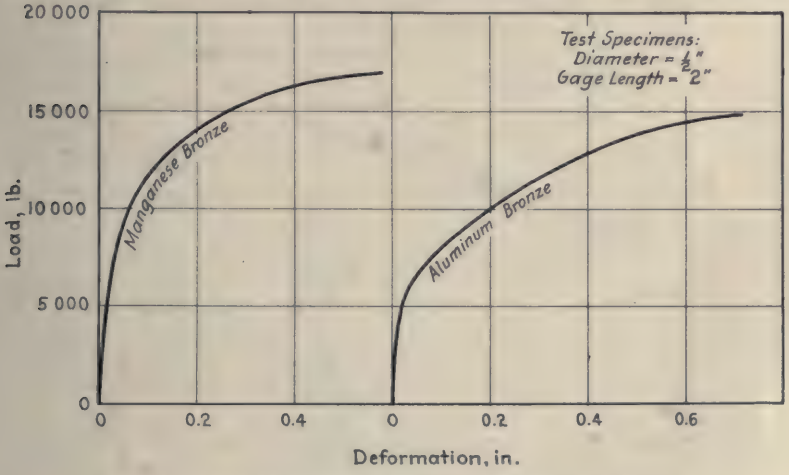


FIG. 1.—Load-Deformation Diagrams for Tension Tests.

asymptote of this parabola is the limit of the safe range of stress to which the material may be exposed indefinitely under reversal, without risk of failure through fatigue."

These precepts have been taken as a guide in making our White-Souther tests for comparing the fatigue resistance of aliminum and manganese bronzes, and while they have not been followed exactly, the departure from them has been on the safe side, so to speak, in that our tests were made with stresses which gave endurances running from a few hundred thousand to six or twenty-three million revolutions, or twice as many reversals of stress.

The material for these tests was cast half in sand and half in a chill, handled by regular methods, and in the form of rods $1\frac{1}{4}$ in. in diameter and 18 in. long. Tension tests were made from one specimen $\frac{1}{2}$ in. in diameter taken from the center of one such bar of each metal, and the results were as follows:

	Manganese Bronze.	Aluminum Bronze.
Proportional limit, lb. per sq. in.	29 200	20 400
Tensile strength, lb. per sq. in.	87 400	76 900
Elongation in 2 in., per cent.	25.0	30.5
Reduction of area, per cent.	25.2	27.6

TABLE III.—RESULTS OF WHITE-SOUTHER ENDURANCE TESTS.

Maximum Fiber Stress, lb. per sq. in.	Revolutions Endured before Fracture.	
	Manganese Bronze.	Aluminum Bronze.
37 100	.....	562 800
	.....	785 000
33 600	248 400	2 067 700
	277 000	2 371 300
30 000	.....	3 687 900
	415 000	3 850 600
	423 200	5 446 900
	454 200	6 809 500
	.....	9 578 800
	.....	23 579 500
26 400	949 500	10 232 400
	1 074 800	13 106 600
	1 239 100	.....
22 900	2 046 600	.....
	2 363 500	.....
	3 378 900	.....
19 400	5 357 500	.....
	6 080 000	.....

The autographic load-deformation curves from which the proportional limits were obtained were carefully copied by tracing, and copies of them are shown in Fig. 1.[1] It might be noted in this connection that the parts of the bars actually tested in both the tension tests and the endurance tests were $\frac{1}{2}$ in. in

[1] In preparing this and similar figures for reproduction, the load-deformation curves have been carefully redrawn with the origin of coordinates in the customary position at the lower left corner, instead of at the lower right corner as on the autographic diagrams from which the authors traced the curves. In arranging these figures for publication, it was found necessary to reduce the deformation scale to half its original value. The accuracy of the proportional-limit determinations was thus actually twice as great as is apparent from the curves here shown.—ED.

diameter, and were originally situated at the centers of the $1\frac{1}{4}$ in. round castings. The results of the White-Souther endurance tests on these alloys are given in Table III, and the curves plotted from them are shown in Fig. 2.

Owing possibly to the fact that the material tested was in the cast condition, and neither rolled nor forged, the results were somewhat irregular, and did not always check each other as well as might be desired. Low results caused by flaws in the specimens, or by undue vibration of the testing machine, were

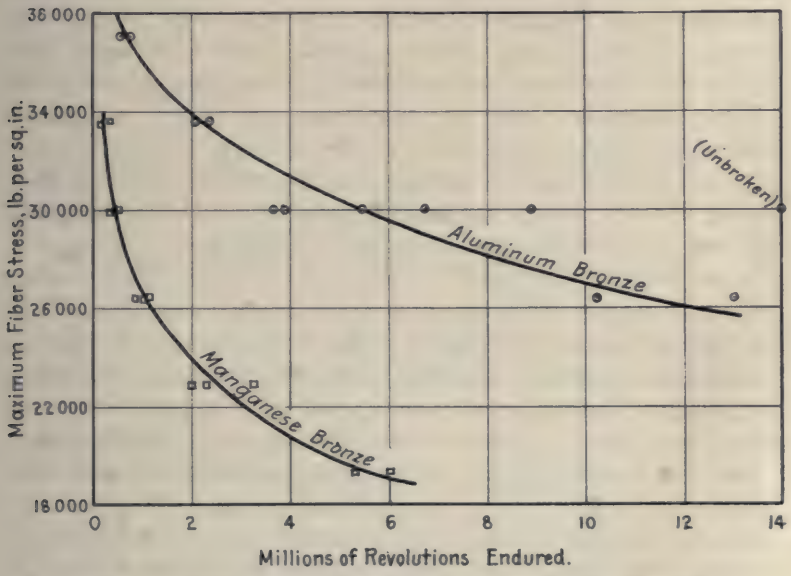


FIG. 2.—Stress-Endurance Diagrams for White-Souther Tests.

disregarded and are not reported here. The one specimen of aluminum bronze which endured over twenty-three million revolutions before failure, at a maximum fiber stress of 30,000 lb. per sq. in., must be considered of abnormal quality, since this extraordinary result was not checked in any other test, and this point was therefore not considered in plotting the curve shown in Fig. 2.

The results show clearly that aluminum bronze, in spite of its comparatively low yield point and proportional limit as

shown in a tension test, is far superior to manganese bronze in endurance of alternating stresses or resistance to fatigue, and therefore its life would be longer in practical use. The recent claims of a well-known Russian engineer, that the proportional limit determines the resistance to fatigue, are shown by these tests not to hold true for all materials, and it is evident that tension tests alone should not be relied upon in determining the comparative values of two materials for engineering uses.

An interesting confirmation of these results is found in the Eighth Report to the Alloys Research Committee of the British Institution of Mechanical Engineers, but here the authors, Messrs. Carpenter and Edwards, took a Swedish Bessemer steel of about 0.35-per-cent carbon, instead of manganese bronze, for comparison with 10-per-cent aluminum bronze. A close similarity was found to exist between these two alloys in microstructure, hardening capacity, resistance to impact, endurance of alternating stresses, elongation and tensile strength. The similarity was also observed in the yield point, provided that "the true, not the primitive, yield point" is taken for the steel. This method of comparing the yield points of these two materials is justified by the authors of the Eighth Report in view of the work of Stanton and Bairstow,[1] showing "that the primitive yield point," or yield point as determined in a tension test, "of a steel is usually an artificial figure, and is due to a stiffening caused by mechanical treatment in preparation" of the test specimen. "Thus they found that the proper yield point of the Bessemer steel mentioned was 15.2 tons per sq. in.," instead of 25.8 tons per sq. in. as was shown by a tension test. In the case of ten different irons and steels tested by Stanton and Bairstow for primitive yield point and maximum range of stress, between tension and compression, which could be endured for one million reversals, the ratio of maximum range of stress to primitive yield point was found to vary between 1.80 and 1.13. It is understood of course that if the primitive and true yield points were identical, this ratio would be 2.00. In the case of 10-per-cent aluminum bronze it was found to be 1.91, which means that there is, in the words of the Eighth Report, a "close approximation of the maximum stress, under which this alloy will bear an unlimited number

[1] *Proceedings*, British Inst. Civil Engrs., Vol. CLXVI, p. 78 (1906).

of reversals, to the stress at the elastic limit as determined in a tensile test." Thus the yield point in tension of this aluminum bronze was found to be 14.8 tons per sq. in., while the range of stress at which it would break after one million revolutions was calculated from several tests to be 28.3 tons per sq. in., from which the ratio 1.91, as noted above, was obtained. This shows, quoting the Eighth Report again, that 10-per-cent aluminum bronze does "not show anything like the same tendency to assume an artificial yield point" that steel shows, and that its "inferiority is thus only apparent, not real" in respect to low yield point. In other words, it is the alternating-stress test that shows the true safe load that a material will stand in practical use, and with aluminum bronze the yield point in tension shows about the same figure, or even lower according to our tests, while with ordinary steel and manganese bronze the yield point as shown in a tension test is decidedly higher than the true safe load to be used in practice.

HEAT TREATMENT OF ALUMINUM BRONZE.

It was noted above in passing that the Alloys Research Committee Report mentioned a similarity between 10-per-cent aluminum bronze and steel in hardening capacity. Both metals, when quenched in water from temperatures above about 800° C., become much harder, have a higher yield point with very little ductility, and lose their duplex microstructure, which is transformed into a uniform mass of grains showing acicular or needle-like markings. This structure is known as "martensite" in steel, while in aluminum bronze it is called the "beta" solid solution. It is well known that to obtain the best possible combination of strength and ductility in a piece of steel, two heat treatments should be given. The first is a quenching treatment to refine the grain and raise the strength and elastic limit, while the second is a cautious annealing to allow some of the normal transformations to take place and develop some ductility, without however giving sufficient time or heat to alter the fineness of the grain. A double heat treatment like this was thought to be worth trying on aluminum bronze, in view of its similarities to steel, to see if the proportional limit could be raised without the loss of too much ductility, and with a possible increase also in the resistance to fatigue.

For some heat-treating experiments of this kind 25 tension test specimens were cast to size in sand from a single melt of our No. 5 aluminum-bronze alloy of the following composition:

Copper	88.63 per cent.
Aluminum	10.67 "
Iron	0.91 "

The specimens varied in diameter between shoulders from 0.493 to 0.521 in., respectively, the length between shoulders being

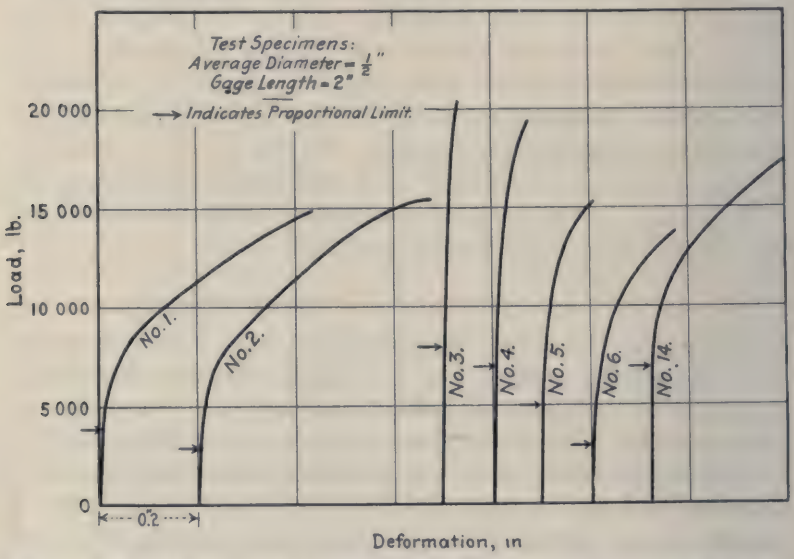


FIG. 3.—Load-Deformation Diagrams for Tension Tests, showing Effect of Heat Treatment.

about 2 in. The total length of the specimens was 9 in., and the diameter of the ends was about $\frac{3}{4}$ in. The gage length of each bar was smoothed a little with a fine file where necessary, but no other machining was done before testing. The tension tests were made with an Olsen autographic machine of 100,000-lb. capacity, but using a poise on the beam which gave a capacity of 50,000 lb., thus increasing the refinement of the stress measurements. Autographic load-deformation diagrams were recorded for each test, and the proportional limits were obtained from these diagrams by noting the point where each curve began to deviate

from its original nearly vertical direction. The yield point as usually determined with dividers would be slightly above this proportional limit in practically every case. Figs. 3 and 4 show accurate tracings of most of the autographic diagrams obtained in this work, and the point taken as the proportional limit is indicated in each case by a small arrow. The curve for specimen No. 1 in Fig. 3 was obtained from one of the bars tested as cast, without any heat treatment. The results obtained were as follows:

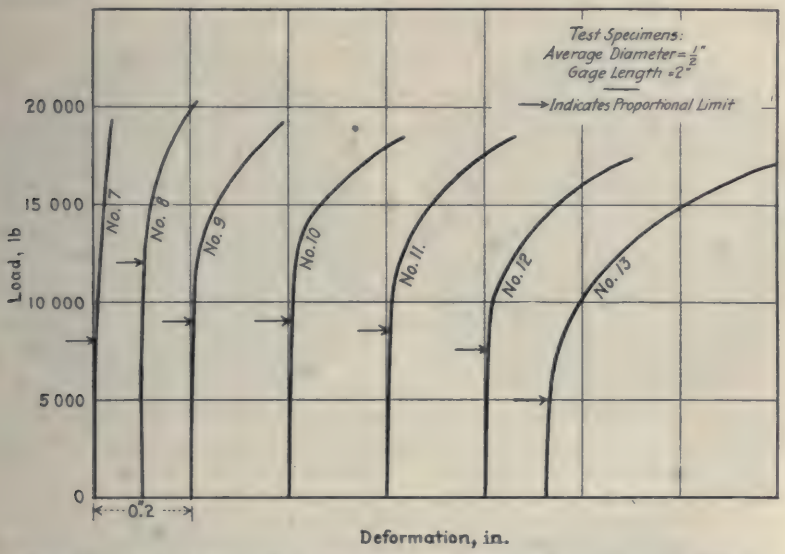


FIG. 4.—Load-Deformation Diagrams for Tension Tests, showing Effect of Heat Treatment.

Proportional limit, lb. per sq. in	19 800
Tensile strength, lb. per sq. in	74 000
Elongation in 2 in., per cent	19.5
Reduction of area, per cent	23.7
Brinell hardness number	100

The hardness in each case was determined with a standard Swedish Brinell machine on a cross-section of one of the ends of the specimen after it had been subjected to the tension test. A load of 500 kg. was used on every specimen except on Nos. 3

and 4, which were so hard that a heavier pressure was required and 3000 kg. were used.

A cross-section for microscopic examination was cut just beyond the shoulder of the bar, and the hardness specimen was taken next to it, so that both, though unaffected by the deformations of the tension test, were yet close enough to the tested portion to have experienced the same heat treatment. Fig. 5 shows the microstructure of this material as cast, the light grains being the alpha constituent, and the dark substance being beta according to the authors of the Eighth Report to the Alloys Research Committee; but according to Gulliver and others, as seems more likely, it is a eutectoid, similar to pearlite in steel, of alpha and gamma. All the photomicrographs presented herewith show polished sections after etching with ferric chloride and hydrochloric acid, and all were taken with a magnification of 200 diameters.[1]

The heat treatment was given in a small electrically-heated muffle, just long enough for the specimens, and about 6 in. wide, with a rounded roof $3\frac{1}{2}$ in. high. A new pyrometer of the Le Chatelier type was used to measure the temperatures, and it was found correct for the melting point of lead. In every heating the test specimens were brought up to the desired temperature slowly with the furnace, and this temperature was maintained constant for a long enough time for thorough diffusion to take place.

Determination of Proper Temperature for Quenching.—It has been stated above that this aluminum bronze can be hardened by quenching from a high temperature, and the first tests were made to determine the proper temperature for alloy No. 5, and to show the effect of a short air cooling before quenching. Specimen No. 2 was quenched from 600° C., and no hardening effect was noticed. Its stress-deformation curve is shown in Fig. 3 and the results from it were as follows:

Proportional limit, lb. per sq. in	15 300
Tensile strength, lb. per sq. in	79 600
Elongation in 2 in., per cent	21.0
Reduction of area, per cent	22.2

[1] In arranging these photomicrographs for publication, it was found necessary to show them at the reduced magnification of 150 diameters.—ED.

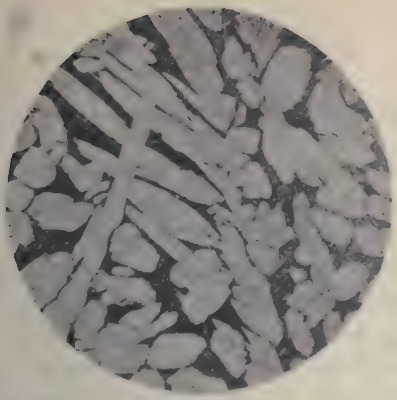


FIG. 5.—Structure of Specimen No. 1, cast in sand, not heat-treated. Light alpha crystals in dark eutectoid.

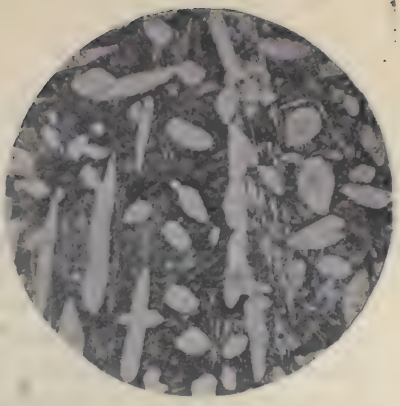


FIG. 6.—A specimen quenched from 700° C. Fewer alpha crystals in acicular beta.

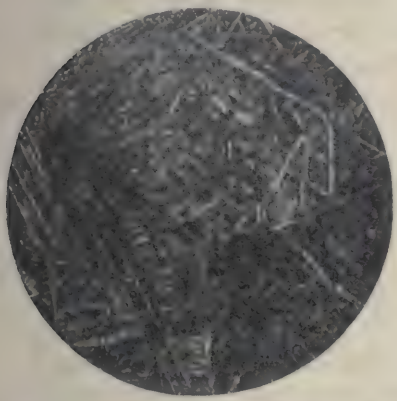


FIG. 7.—A specimen quenched from 900° C. Three grains of acicular beta. Alpha entirely dissolved.

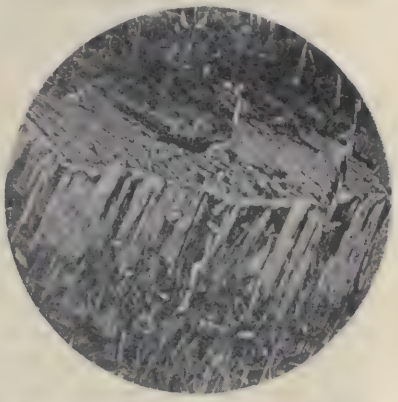


FIG. 8.—Specimen No. 3, cooled in air 10 seconds before quenching. Growth of alpha along edges of beta grains.

FIG. 9.—Specimen No. 4, cooled in air 20 seconds before quenching. Alpha appears inside beta grains.

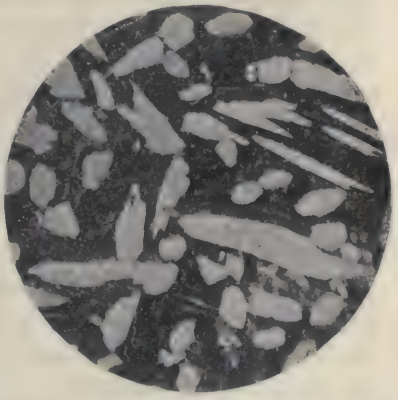


FIG. 10.—Specimen No. 5, cooled in air 30 seconds before quenching. Alpha collecting in big crystals like Fig. 5.

A specimen quenched from 700° C. was spoiled by a casting flaw, but a photomicrograph (Fig. 6) showed that this temperature was not high enough. Although the dark constituent here shows the acicular markings typical of the beta solution, and has increased decidedly in quantity as compared with Fig. 5, there are still many crystals of soft ductile alpha that were not dissolved. Specimens quenched from 800, 900, and 1000° C. were alike in structure, as is illustrated by Fig. 7., where the junction of three grains is shown, and all are composed of the beta solution with acicular markings well developed, and with all the alpha completely dissolved. A temperature of 850° C. was adopted as the proper quenching heat for this material, and one of the specimens so quenched had the following physical properties:

Proportional limit, lb. per sq. in	39 800
Tensile strength, lb. per sq. in	97 400
Elongation in 2 in., per cent	1.0
Reduction of area, per cent	None
Brinell hardness number	240

The stress-deformation curve for this specimen is that of specimen No. 7 in Fig. 4.

Effect of Air Cooling before Quenching.—Four tests were made to find the results of air cooling for different periods between the heating at 850° C. and the quenching in water. Specimen No. 3 was thus air cooled for 10 seconds before quenching; specimen No. 4 for 20 seconds; Specimen No. 5 for 30 seconds; and specimen No. 6 for 45 seconds. The stress-deformation curves are shown in Fig. 3, and indicate plainly that this treatment lowers the elastic limit of the quenched material with only a slight increase in ductility. The curve of specimen No. 6 would probably have been longer if a flaw had not existed in the casting. The physical properties of these bars are given in Table IV.

These tests show that if 10 seconds elapse between the withdrawal of a small specimen of this alloy from a furnace at 850° C. and its immersion in water, the quenching effect will not be lessened. The allowable time will of course vary with the size of the specimen and the temperature of the air. Nothing is gained by intentional air cooling before quenching. The

microstructures of these specimens, Figs. 8, 9 and 10, are rather interesting. Fig. 8 shows the boundary between two grains in specimen No. 3, where the light alpha constitutent is beginning to separate, or be precipitated, from the acicular beta. In this case there was not enough air cooling for very much alpha to form, but in Fig. 9, showing specimen No. 4, which was air cooled 20 seconds before quenching, the alpha crystals are much more extensively developed not only at the boundaries but inside the beta grains as well. Fig. 10, showing specimen No. 5, is a close approach to the original cast structure of this material (Fig. 5), with less alpha however and plainer acicular markings in the dark constituent.

TABLE IV.—RESULTS OF TESTS ON AIR-COOLED SPECIMENS.

Specimen No.	Time of Air Cooling, before Quenching, seconds.	Proportional Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	Brinell Hardness Number.
3.....	10	40 500	105 200	1.0	0.8	262
4.....	20	33 200	91 500	2.0	5.7	229
5.....	30	23 600	73 200	5.0	9.4	150
6.....	45	15 700	72 500	7.0	12.2	...

Effect of Reheating after Quenching, and Cooling in Air and Furnace.—Some tests were next made on specimens reheated after quenching from 850° C. A few preliminary treatments of small specimens followed by microscopic examinations indicated 650° C. as a good temperature for the annealing. Two specimens were therefore reheated, after quenching, to this temperature for 15 minutes, and one was then withdrawn from the furnace and air cooled, while the other was left to cool very slowly in the furnace. Physical tests on these bars gave the following results:

	AIR COOLED.	COOLED IN FURNACE.
Proportional limit, lb. per sq. in.....	35 100	42 900
Tensile strength, lb. per sq. in.....	86 600	94 000
Elongation in 2 in., per cent.....	13.0	12.5
Reduction of area, per cent.....	14.9	12.3
Brinell hardness number.....	130	143

The stress-deformation curve of the air-cooled specimen is that for specimen No. 14, Fig. 3, and of the furnace-cooled specimen No. 11, Fig. 4. These results are very surprising in that the specimen which was cooled faster (in air) was less strong, more ductile, softer, and lower in proportional limit than the specimen cooled very slowly in the furnace. This remarkable fact was checked several times by hardness determinations on different small specimens similarly heat-treated, and always the furnace-cooled specimen was harder than the air-cooled one. As the object of this work was to find a way of raising the elastic limit of this material without destroying its ductility, it seemed

TABLE V.—RESULTS OF TESTS ON SPECIMENS QUENCHED AND REHEATED AND COOLED SLOWLY IN FURNACE.

(See also Fig. 11.)

Specimen No.	Temperature to which Reheated after Quenching, deg. Cent.	Time of Heating, minutes.	Proportional Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	Brinell Hardness Number.
8.....	500	30	56 700	96 100	5.5	9.1	158
9.....	575	20	46 100	94 100	8.0	11.2	143
10.....	610	15	43 400	96 700	10.5	12.6	140
12.....	650	15	39 300	91 200	14.0	18.5	143
13[a].....	700	15	23 400	80 000	23.0	21.7	104

[a] After cooling from 700 to 550° C., specimen was withdrawn from furnace and cooled in air.

evident that the furnace-cooled specimen came closer to satisfying this requirement than the one cooled in air.

A final series of tests was made on quenched specimens reheated to different temperatures between 500 and 700° C., and cooled slowly in the furnace. Specimen No. 8 was heated to 500° C. for about 30 minutes; specimen No .9 to 575° C. for about 20 minutes; specimen No. 10 to 610° C. for 15 minutes; specimen No. 12 to 650° C. for 15 minutes; and specimen No. 13 to 700° C. for 15 minutes, and then this specimen was withdrawn from the furnace at about 550° C., and cooled in air. The results of tests on these bars are given in Table V.

These results are plotted in Fig. 11, together with the results from specimen No. 11, previously given, a duplicate of

No. 12 in respect to heat treatment; and also with the results from specimen No. 3, considered as a quenched bar without the second heat treatment, to form the starting points for the curves.[1]

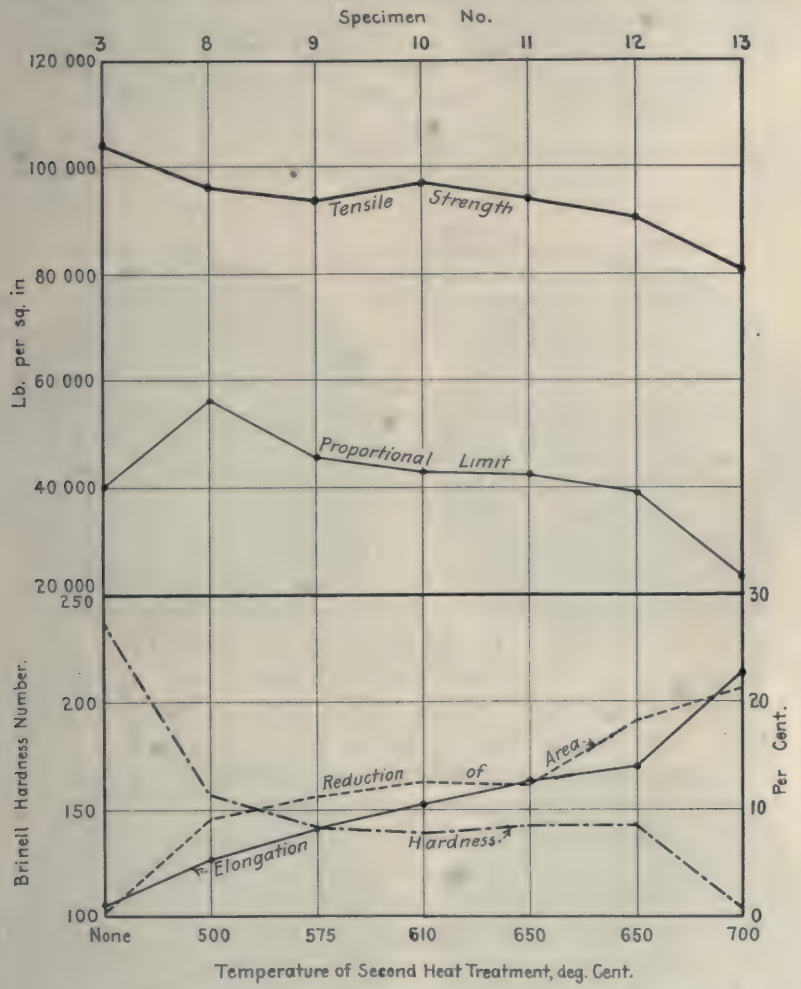


FIG. 11.—Physical Properties of Specimens Annealed after Quenching from 850° C.

A duplicate of specimen No. 10 was also tested, but is not included here because its results were almost exactly the same

[1] For the physical properties of specimen No. 3, see Table IV.

as those from specimen No. 10. The stress-deformation curves of these specimens are shown in Fig. 4.

Some specimens which had been heat-treated with those whose results are given in Table V were sent to the Bureau of Standards and there tested. Small flaws were reported in all of them, however, and therefore the results were not as good as they should have been. One specimen heat-treated similarly to specimen No. 12 was reported by the Bureau to have given the following results, which are in good agreement with those found here:

Proportional limit, lb. per sq. in	40 000 – 45 000
Tensile strength, lb. per sq. in	88 500
Elongation in 2 in., per cent	15.0
Reduction of area, per cent	17.3

The microstructure of specimen No. 8, shown in Fig. 12, has a good deal of the acicular structure remaining, but with some feathery precipitation of alpha in fine particles along the grain boundaries, and a few larger masses within the grains. In the following photomicrographs the acicular structure has entirely disappeared: specimens Nos. 9 and 10, Figs. 13 and 14, respectively, had practically the same structure, but with a few larger grains in specimen No. 9 due perhaps to the slightly longer period of annealing. It will be noted that both these photomicrographs are rather indistinct, and show a very fine-grained structure of light alpha crystals with a dark constituent between them. The indistinctness is partly due to the fineness of the structure, partly to the great difficulty of polishing this material without scratches, and partly to the fact that the etching reagent shows a much greater tendency to attack these small crystals of alpha than it does with the ordinary coarser structures. Specimen No. 11 had a structure similar to these, and the structure of No. 14, the air-cooled specimen, is shown in Fig. 15. This structure is much more distinct and apparently coarser than those of the furnace-cooled bars heated to the same temperature. It may be seen, however, that the different grains show different degrees of fineness of their crystals, and the greater coarseness of the air-cooled sample is thus probably more apparent than real. But the greater distinctness of the air-cooled structure cannot

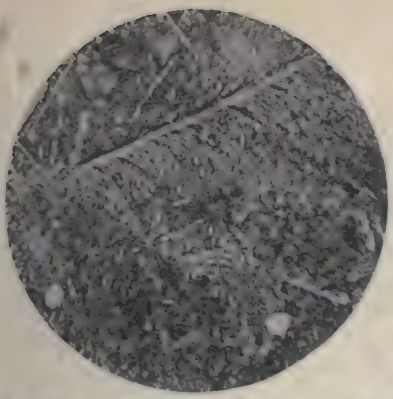


FIG. 12.—Specimen No. 8, quenched, reheated to 500° C. and cooled slowly. Beta slightly changed.

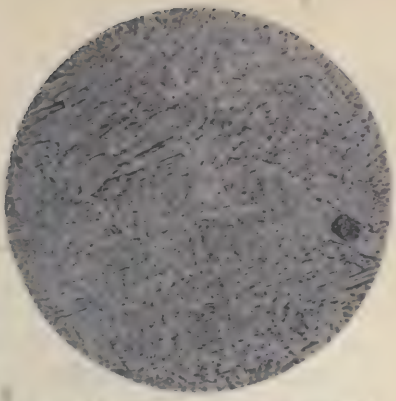


FIG. 13.—Specimen No. 9, quenched, reheated to 575° C. and cooled slowly. Very fine alpha and eutectoid.

FIG. 14.—Specimen No. 10, quenched, reheated to 610° C. and cooled slowly. Indistinct structure.

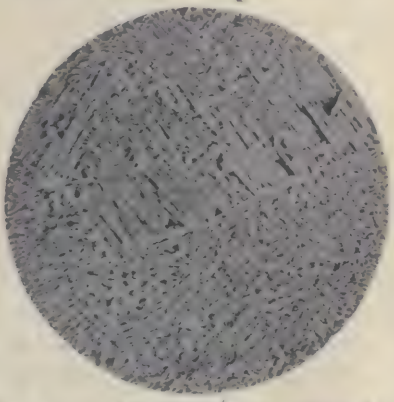


FIG. 15.—Specimen No. 14, quenched, reheated to 650° C. and cooled in air. Structure more distinct.

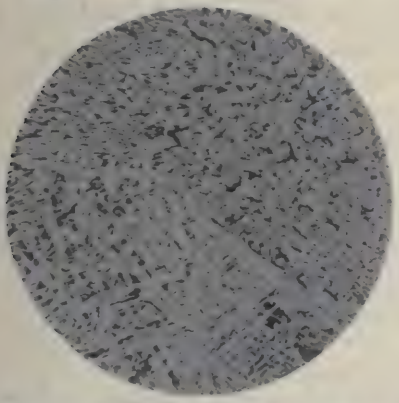


FIG. 16.—Specimen No. 12, quenched, reheated to 650° C. and cooled slowly. Coarser structure.

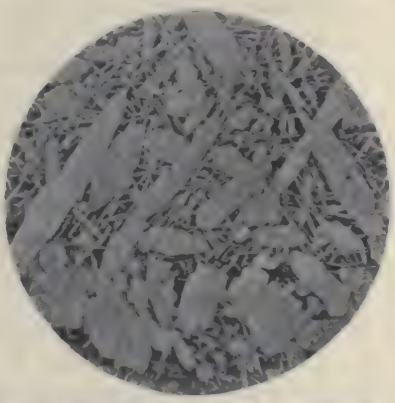


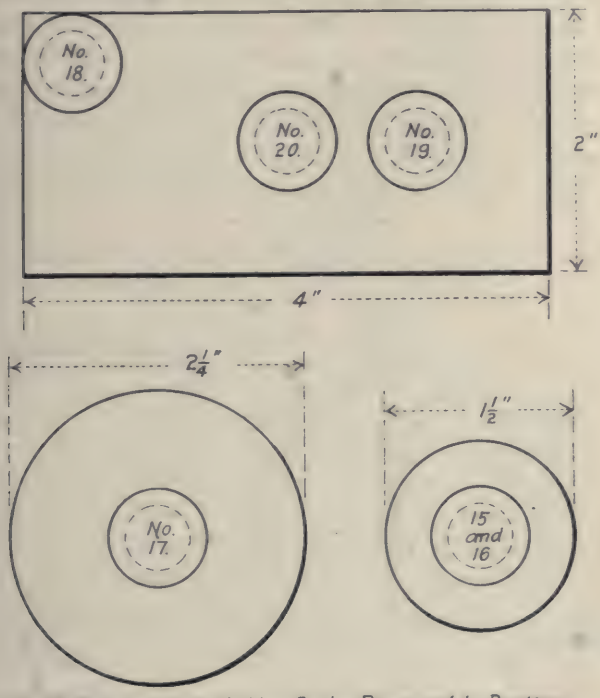
FIG. 17.—Specimen No. 13, quenched, reheated to 700° C. and cooled slowly. Structure much coarser.

be questioned, although all the samples were polished and etched in the same way. Perhaps the very slow cooling in the furnace through low temperatures causes a certain diffusion of the dark constituent into the lighter alpha, as some action like this would explain the greater hardness of the furnace-cooled metal as well as the indistinctness of its structure. Or it is possible that there is some hardening transformation in either the alpha or the dark constituent or both at a low temperature, below a red heat, which is prevented or decreased by air-cooling. The Eighth Report to the Alloys Research Committee mentions an increase in yield point and decrease in ductility that takes place in this alloy after annealing at 400° C., but with no apparent change in the microstructure.

Specimen No. 12, supposed to be a duplicate of specimen No. 11, had quite a decidedly coarser structure, and its physical properties also indicated a more complete annealing than specimen No. 11 had received. Its structure is shown in Fig. 16, where corners of four different grains are included. Each grain has its own typical arrangement of crystals, but on the whole the structure appears similar in type to that of specimen No. 9 or No. 10, although the texture is coarser. In Fig. 17 the structure of specimen No. 13 is shown, and it is seen to be entirely different, just as its physical properties are so different from those of specimens Nos. 9 and 10, and even No. 12. Coarse crystals of alpha are here present, as in the cast metal, but the ground-mass shows also some finer needles of alpha, having an appearance somewhat like Fig. 15. The coarse crystals probably grew while this bar was in the furnace at temperatures between 650 and 700° C.; and when it was air cooled below 550° C. a little more alpha was precipitated from the dark constituent, but did not have chance to unite with the larger crystals because of the low temperature, and hence remained in finer particles along the cleavage planes of the dark constituent.

This work on heat treatment has been described in some detail because it is not believed to be generally known that aluminum bronze is nearly as susceptible as steel to improvement in this way. At the time when the experiments were made the double heat treatment, that is, quenching followed by tempering, was thought to be something new for bronzes, but subsequently

a paper by Messrs. Portevin and Arnou[1] was found, where similar results were reported by these investigators on 10-percent aluminum bronze treated by the same processes that we have described above. They however tried a wider range of



Note: The Small, Full-Line Circles Represent in Position and Diameter the Test Bars from which the Specimens Actually Tested, Shown by the Dotted Lines, were Machined.

FIG. 18.—Location of Tension Test Specimens Cut from Heat-Treated Castings. (Dotted circles show parts tested.)

tempering temperatures than we used, and their report emphasizes the remarkable fact that a tempering to only 400° C. made their specimens actually harder than they were after quenching. Reheating to above 400° C. was necessary to obtain any softening effect after quenching.

[1] "Sur le Revenu des Bronzes d'Aluminium," *Comptes Rendus*, French Academie des Sciences, Vol. 154, No. 8, p. 511, February 19, 1912.

Application of Heat Treatment to Larger Castings.—The favorable results obtained by a double heat treatment of the small cast-to-size bars naturally raised the question as to whether a similar improvement could be brought about when the method was applied to castings of larger size. A rectangular block 2 by 4 by 9 in. was cast for an experiment to determine this point, and tests were made also with one bar $2\frac{1}{4}$ in. in diameter and two bars about $1\frac{1}{2}$ in. in diameter. These represent cross-sections as large as the average castings for gears, bearings, etc., though of course the total mass of metal was not as great in any of them as in many of the regular castings made of aluminum bronze

TABLE VI.—RESULTS OF TESTS ON SPECIMENS OF LARGER SIZE SUBJECTED TO DOUBLE HEAT TREATMENT.

Specimen No.	Original Size.	Temperature of Second Treatment, deg. Cent.	Proportional Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.
15.....	$1\frac{1}{2}$ in. diam.	600	53 600	93 300	10.0	13.5
16.....	$1\frac{1}{2}$ in. diam.	630	43 300	88 500	13.0	14.2
17.....	$2\frac{1}{4}$ in. diam.	650	34 600	74 000	14.5	17.6
18.....	2 by 4 in. block.	650	40 800	85 100	12.5	15.7
19.....	2 by 4 in. block.	650	39 700	78 200	9.5	15.0
20.....	2 by 4 in. block.	650	40 200[a]	74 700	9.0	13.5

[a] This value is for yield point instead of proportional limit, since no stress-deformation curve was obtained for specimen No. 20.

in our foundry. The pieces of the sizes mentioned were quenched and reheated to 650° C., except the $1\frac{1}{2}$-in. round bars which were tempered at 600 and 630° C. Afterward test bars were cut from the centers of all the pieces, and also from the corner and midway between the center and edge of the rectangular block, as shown by Fig. 18. The specimens were machined from these test bars to the same size as the cast-to-size bars previously treated, and tested in the same way. The results are given in Table VI, the bars being numbered in the same way as their locations in Fig. 18, and their load-deformation curves (except for specimen No. 20) are shown in Fig. 19.

These results indicate that the double heat treatment improves large castings in about the same way as small bars,

although the values finally obtained with the large castings are of course not so good as those obtained from small ones. The great trouble in treating a large casting seems to lie in the impossibility of chilling it quickly enough in water to refine the grain properly. The scleroscope hardness after quenching should be between 55 and 60. Fig. 20 shows the microstructure of a $1\frac{1}{2}$-in. round bar that was properly quenched, but to obtain this result it was necessary to be very quick with the quenching. Fig. 21 shows the structure of a bar similar in all respects to

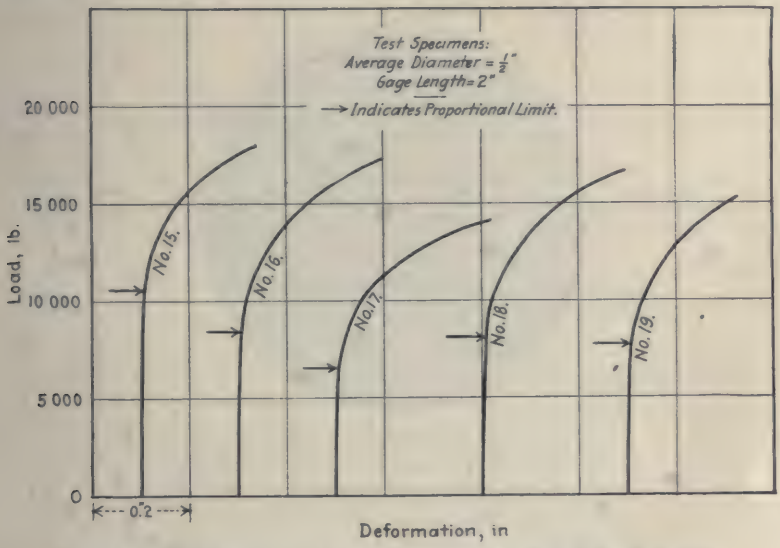


FIG. 19.—Load-Deformation Diagrams for Larger-Size Specimens Subjected to Double Heat Treatment.

specimen No. 16, and the structures of specimens Nos. 17, 18 and 20 are shown by Figs. 22, 23 and 24, respectively. Although the latter structures are much coarser than that seen in a small treated casting, they are nevertheless finer and quite different from that of a casting that was not heat treated (Fig. 5). Specimen No. 17 had a coarser structure than specimens Nos. 18 or 20, probably because it was not quenched so quickly, and that is the reason its proportional limit was lower and its ductility higher than in the other specimens. It should be noted that the differences between the center and edge of the rectan-

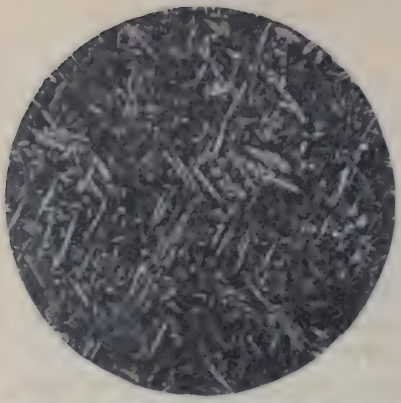


FIG. 20.—Acicular structure of bar 1½ in. in diameter, quenched in water from 950° C.

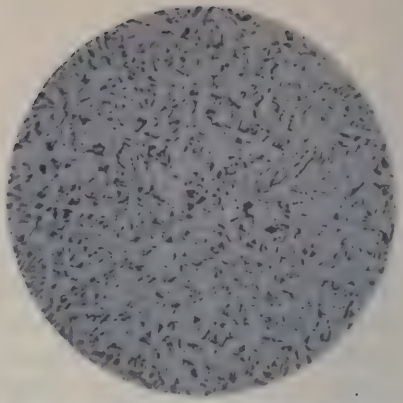


FIG. 21.—Similar bar reheated to 630° C. (Specimen No. 16), showing fine structure.

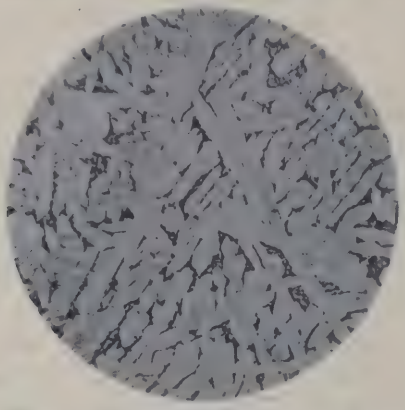


FIG. 22.—Comparatively coarse structure of Specimen No. 17, from center of bar 2¼ in. in diameter, after double heat treatment.

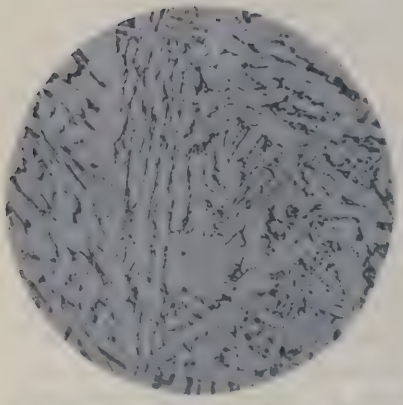


FIG. 23.—Structure of Specimen No. 18, from corner of 2 by 4-in. casting after double heat treatment.

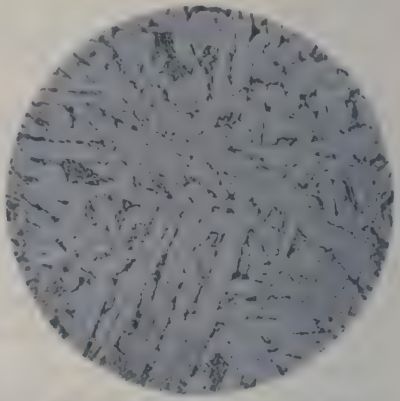


FIG. 24.—Structure of Specimen No. 20, from center of same casting as Fig. 23.

gular block are not excessive either in structure or properties and at both locations there are marked improvements in both proportional limit and tensile strength over the properties usually found in a large untreated casting.

Effect of Heat Treatment on Endurance of Aluminum Bronze.—The chief advantage gained by heat treatment of aluminum bronze as described above is the raising of the proportional limit without the sacrifice of a necessary amount of ductility. This might be a great advantage in itself for some purposes, but, as

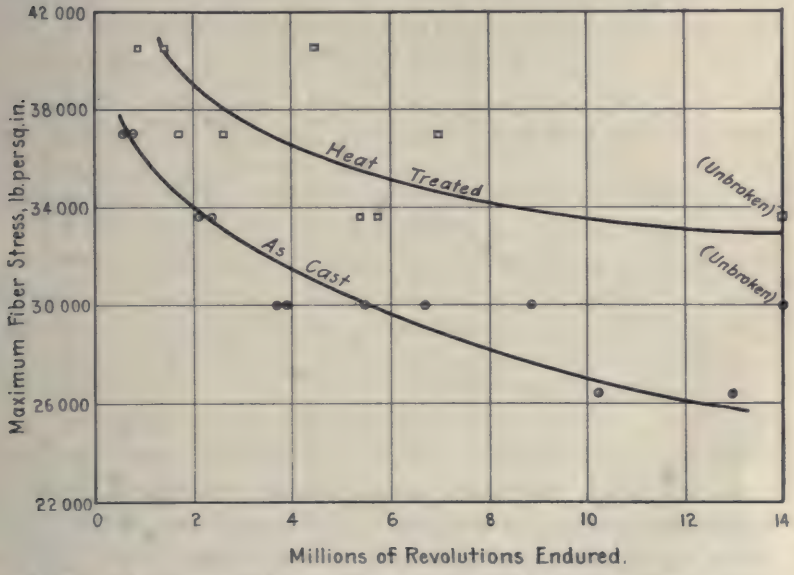


FIG. 25.—Stress-Endurance Diagrams for White-Souther Tests on Aluminum Bronze.

stated in the first part of this paper, for most engineering uses the life of a material under alternating stresses is more important than its capacity for resisting a single slowly applied force. Hence the good results in tension obtained from aluminum bronze by the double heat treatment would be of much greater interest, if they could be shown to imply a better endurance of alternating stresses as compared with that of an untreated casting. A few tests of heat-treated bars have been made for this purpose with the White-Souther endurance testing machines,

the bars, about 1½ in. round, having been cast in the usual way, and heat treated before machining to test specimen size. The results are plotted as shown in Fig. 25, together with the endurance curve for the same alloy untreated; the numerical values are given in Table VII.

The last value in Table VII, namely, 29,750,000, is probably somewhat better than could ordinarily be expected for this material, but all the results are a decided improvement over those obtained from the same alloy as cast, without heat treat-

TABLE VII.—ENDURANCE TESTS OF HEAT-TREATED ALUMINUM-BRONZE SPECIMENS.

Maximum Fiber Stress. lb. per sq. in.	Revolutions Endured before Fracture.
40 600	882 600
	1 431 800
	4 538 400[a]
37 100	1 730 700
	2 662 100
	7 085 000
33 600	5 432 100[b]
	5 770 800
	29 750 900

[a] See Fig. 21 for photomicrograph.
[b] This bar was machined 0.003 in. too small.

ment, and show that the finer grain of the heat-treated material gives a greater endurance of alternating stresses as well as higher strength and proportional limit in tension. While many steels would, of course, give better results than these, no non-ferrous alloy has ever been reported, so far as we know, that would approach aluminum bronze in this respect. When it is remembered, moreover, that all the results reported above were obtained from unworked bars, neither forged nor rolled, they will, we feel sure, be found of sufficient interest to warrant their presentation before the Society.

DISCUSSION.

Messrs. Corse and Comstock.

MR. W. M. CORSE and MR. G. F. COMSTOCK (*presented in written form and read by Mr. Corse*).—In addition to the references given in footnotes in the paper, we should like to mention also a paper on "The Endurance of Metals," by Eden, Rose, and Cunningham.[1] This paper and the discussion published with it are full of valuable information in regard to endurance tests on steels with machines of various types, including the rotating cantilever type used by us. Results on various wrought irons and steels, including axles and tool steel, are quoted in this paper, at slower speeds however than used by us, and the maximum fiber stresses giving fractures after one million alternations are reported to run from 26,000 to 36,000 lb. per sq. in. This is near our results on manganese bronze and below those on aluminum bronze; but we have tested many titanium-treated steel rails that stood 30 or 40 million revolutions at higher stresses than any used for these bronzes. Some steel axles tested by us have however given about the same results as aluminum bronze.

It will be noticed that our endurance curves do not show by horizontal endings any definite limiting stress at which fracture would never take place no matter how great the number of repetitions might be, and thus we have perhaps failed to show the "safe range of stress" quoted in our paper from Rosenhain's book. The authors of the paper on "The Endurance of Metals" seem to feel considerable doubt as to the existence of such a stress, and it is brought out clearly in the discussion that the value arrived at for the limiting stress depends as much on the method of plotting the curve, that is, on the relation between the scale chosen for the stresses and the scale for the number of repetitions endured, as on the values actually obtained by experiment. While these authors emphasize the very great uncertainty of the determination of this limiting stress, and show that a horizontal ending to a stress-revolutions

[1] *Proceedings*, (British) Institution of Mechanical Engineers, 1911, Parts 3 and 4, pp. 839 to 974.

Messrs. Corse and Comstock. curve does not necessarily mean that the limiting stress is not below that point, one of them states that for practical purposes the ordinate of the curve at two or three million revolutions might be regarded as the maximum safe stress. In our opinion this is too small a number of revolutions, and about ten million instead of two or three million should be the minimum number required.

The tests and curves given in our paper are not intended to have horizontal endings, or to show the maximum safe stresses at which an infinite number of revolutions would not break the respective materials; but we have in each case compared two materials under exactly similar conditions, using low stresses that gave large endurances in order to imitate as closely as practicable the stresses that might be encountered in ordinary engineering practice.

Mr. Moore. MR. H. F. MOORE.—The tests of bronze recorded in this paper seem to be important, not merely as tests of particular metals, but also as a contribution to the available data for repeated stress tests. The number of repetitions is higher than is found for most recorded tests. The speaker wishes to emphasize the fact brought out in the latter part of the paper, that the curves for the endurance tests do not show horizontal endings, and to point out further that the results do follow fairly closely an exponential relation, the stress at failure varying inversely as the eighth root of the number of repetitions.

Mr. Devries. MR. R. P. DEVRIES.—The authors discuss primarily the elastic limits of the aluminum and manganese bronzes. Now we have found in working with steel springs that the elastic or proportional limit is not always a good criterion of the resistance of the material to alternating stress. I have found this to be true in both repeated-stress and alternating-stress testing. In testing semi-elliptic springs, we have observed that while the material might show a high elastic or proportional limit, the endurance of the spring as a whole was quite low; in fact, we finally concluded that the ductility is an important factor in determining the resistance of springs to repeated stresses. I should like to ask the authors whether they have compared the results on their endurance tests with the ductility of the specimens rather than with the elastic limit.

There is still another point I should like to mention. The authors indicate that they have come to some conclusion as to the meaning of the terms "primitive yield point" and "true yield point." Might I ask for a further explanation of their conclusion in this respect? Mr. Devries.

MR. CORSE.—I am glad to see that some one else has had difficulty in understanding the distinction between those terms, because it took us some time to understand it ourselves. The idea seems to be that the primitive yield point is the yield point as determined in a tension test, and is what is ordinarily called merely the "yield point." The word "primitive" is added possibly because this is the first quality noticed in making a test, and because the true yield point, as determined by an alternating stress test, is found to be a different value. The point emphasized in regard to aluminum bronze is that it gives in a tension test a value closer to the "true" yield point than other metals. For instance, in testing this alloy in tension, the low "primitive" yield point is noticed at once, while the ultimate strength and elongation are about the same as in manganese bronze. When the "true" yield points are determined by alternating stress tests, however, the manganese bronze is found to be much inferior to the aluminum bronze. Mr. Corse.

MR. DEVRIES.—Is that after treatment? Mr. Devries.

MR. CORSE.—No, in any case; with material in the cast condition, the yield point as shown in a tension test is generally higher than that shown by alternating stress tests; and in manganese bronze it is nearly always higher than in aluminum bronze, although the ultimate strengths may be the same. Mr. Corse.

An engineer's first impression is that a material which shows a low so-called "primitive" yield point in tension is not as good as a material which shows a higher "primitive" yield point in tension. The idea is that in such positions as in a rotor, for instance, where the forces are wholly centrifugal, the yield point value is very important because if the working stress exceeds this, a permanent set will be started which will cause the rotor to hit the outside casing. An instance was given to me by Mr. Capp, of the General Electric Co., where a man had attempted to use the primitive yield point instead of the true elastic limit in rotor design, and got into trouble. When the

Mr. Corse. true yield point is determined from an endurance test, this figure would generally be correct in such cases, and would be found, for most materials, to be quite different from the primitive yield point, so that it is not fair to judge a metal by its primitive yield point alone.

The value of 2.00 for the ratio of maximum range of stress (permissible for an endurance of one million or more cycles) to primitive yield point, if the primitive and true yield points are identical, is derived from the fact that this maximum range of stress is twice the true yield point. Thus if this range is found to be 24,000 lb. per sq. in., it would extend from minus 12,000 or 12,000 lb. per sq. in. in compression to plus 12,000 or 12,000 lb. per sq. in. in tension, and the true yield point would be 12,000 lb. per sq. in. If the primitive yield point found in tension were 12,000 lb. per sq. in., like the true yield point, it is readily seen that the ratio of maximum range of stress to primitive yield point is 2.00; and if the primitive yield point is higher, as is nearly always the case, this ratio becomes less than 2.00. Manganese bronze and most other metals except aluminum bronze show a primitive yield point in a tension test which is considerably higher than the true yield point shown by endurance, and hence the ratios for such materials are considerably less than 2.00. This theory of the "true" and "primitive" yield points must be ascribed entirely to Stanton and Bairstow, and the above is offered only in explanation of their ideas, which were not clear to me for some time after I first read them.

Mr. Moore. MR. MOORE.—The remarks of the last speaker concerning the unreliability of the primitive yield point, the elastic limit, and the proportional limit as criteria for strength of materials under repeated stress are worthy of emphasis. In this connection it is interesting to note that an examination of the records of the Watertown Arsenal and of the University of Illinois Materials Testing Laboratory shows several series of tests in which failure under repeated stress has occurred at stresses lower than the primitive proportional limit as determined by an extensometer test.

Mr. McAdam. MR. D. J. MCADAM, JR.—I should like to call attention to the difficulty of measuring the actual stress in any endurance tests, and especially in tests on the White-Souther endurance

machine. In this machine, the specimen is held in a bearing 3 or 4 in. in length and the weight is applied at the end of a cylinder about 8 in. long, as I remember. If this cylinder is not lined up exactly true, there will be vibration when the specimen is rapidly revolved in testing. The stresses due to this vibration will be of varying amount and difficult to calculate. Mr. McAdam.

Even if the specimen is in exact alignment, and the elastic limit is not reached, the specimen will be bent slightly by the action of the weight at the end. This bending, combined with the rapid rotation of the specimen, will induce stresses, probably of a torsional nature, whose amount will depend on the modulus of elasticity and the speed of revolution.

In tests at stresses below the elastic limit, a slight variation of the stress will cause a great variation in the number of cycles endured by the metal. It seems evident, therefore, that careful consideration should be given to the subject of actual stresses in tests by the White-Souther machine.

MR. DEVRIES.—The Bureau of Standards has not published as yet the results of its tests with the White-Souther machine, probably because it is felt that it would be better to experiment first with some other type of machine. A considerable amount of testing with this machine has been done, however, and in that connection we have noticed a rather peculiar thing. When a specimen is first put in there may be appreciable vibration while testing, even if the specimen evidently has been turned round and true and carefully inserted in the machine. But in these cases, strange as it may seem, the results are not by any means the lowest as regards endurance. In other words, we have used a good many specimens which showed so much vibration that we felt we ought not to test them, but have found in some cases that they showed 50 per cent greater endurance than that of specimens which apparently were running quite smoothly. This is an important point because when the machine is not running true, there must be slight impact on the specimen even when the weight is going up and down through only a small vibration. Mr. Devries.

MR. MOORE.—I wish to confirm Mr. Devries' statement as to the behavior of the White-Souther machine, as judged by our experience. Mr. Moore.

Mr. Comstock. MR. COMSTOCK.—In answer to Mr. Devries' question relative to ductility, we have not compared the endurance directly with the ductility of the specimen, but our general experience in testing rails has been that those of high ductility and low elastic limit do not show as good endurance as those of low ductility and high elastic limit. We have not compared the endurance with the ductility in bronzes.

AN INVESTIGATION LEADING TO SPECIFICATIONS FOR BRASS CONDENSER TUBES.

By A. E. White.

SUMMARY.

A number of manufacturers of condensers were appealed to by a large operator of turbines for relief from the very frequent splitting of condenser tubes. None was given. In fact, no promise even of possible relief was offered, for the statement was made that splitting was an inherent property to which tubes were subject, and that the trouble would have to be endured in spite of any desire to adequately overcome it.

This paper briefly explains the cause of the trouble. Also the proper chemical composition, mechanical treatment and heat treatment which should be given tubes so that they will not be subject to such a defect, are discussed.

As a conclusion, specifications for condenser tubes are prented, which have obviated the troubles previously encountered.

AN INVESTIGATION LEADING TO SPECIFICATIONS FOR BRASS CONDENSER TUBES.

By A. E. White.

The frequent splitting of brass condenser tubes in service had for some time been giving The Edison Illuminating Co., of Detroit, Mich., so much trouble that in June, 1913, they asked the writer to make such an investigation as would show the causes of the trouble and, if possible, discover a remedy.

Over two and one-half years have elasped since the recommendations submitted were acted upon. In all of that time, no instance of a failed tube has come to light, although in the spring of 1913 it was not uncommon to find days when as many as five tubes were either plugged or removed because of failure. In view of these conditions and appreciating that in this plant over 20,000 tubes are in daily service, it seems evident that the real cause of the trouble was ascertained. It is therefore felt that the specifications which accompany this paper may be of some service; either to other users of surface condensers, or else to some one of the committees of the Society when preparing specifications for such material

In the investigation made in compliance with the above instructions, it was attempted to accomplish three things:

1. To account for the splitting of certain brass condenser tubes in service;

2. To determine the proper chemical composition, mechanical treatment, and heat treatment which should be given to the tubes; and

3. To formulate such specifications as would be of material assistance in the purchase of tubes.

Cause of Splitting.

When the problem of discovering the cause of the splitting of the brass tubes was first considered, it was thought that such splitting might be due to the presence of such impurities as

antimony, bismuth, iron, or other metals of a like nature. It was also felt that the proportions of the copper and the zinc might be at fault. In order to determine the validity of this hypothesis, a chemical analysis of a number of tubes for copper, zinc, lead, iron, antimony, bismuth and arsenic was made.

This examination showed the tubes as a lot to be of quite uniform composition, and gave indications that the manufacturer was working on a basis of 60 per cent of copper and 40 per cent of zinc as his formula. This corresponds to what is known as Muntz metal. It also showed that the quantities of the so-called impurities, which consisted of lead, iron, arsenic, antimony and bismuth, were exceedingly low, and, therefore, the splitting of the tubes could in no way be laid at the door of too high percentages of impurities. In fact, if the alloy employed in the production of these tubes had been properly manipulated in manufacture, it would have produced tubes reasonably satisfactory, but possibly not of the highest quality. This statement is made because it is felt that a 60–40 mixture will not produce as satisfactory a tube as will a 70–30 one.

When it was thus found that the cause of the splitting could not be laid to the chemical composition in the tubes, an additional investigation, subsequently carried out, conclusively indicated that the trouble was due to faulty manufacture. Excessive pinching during the drawing, insufficient annealing between the drawings, or an omission of annealing or incomplete annealing after the final drawing, were among the conditions responsible for the splitting. The trouble, in fact, may be caused by any one, or by any two, or by all three of these conditions. Thus, if drawn too much, or if drawn properly but not sufficiently annealed, the metal is in a state of molecular strain, and its efforts to return to a condition of equilibrium result in a fracture or split in the course of time.

CHEMICAL COMPOSITION.

It is evident that the chemical composition as well as the mechanical and heat treatments have much to do with the life of a condenser tube. Looking at the aspect of the matter from the chemical standpoint, it is believed, for several reasons, that for fresh unpolluted water the best service can be obtained from

condenser tubes containing 70 per cent of copper and 30 per cent of zinc. An Admiralty mix, consisting of 70 per cent of copper, 29 per cent of zinc and 1 per cent of tin, is especially good for salt and polluted water, as well as a mix containing 80 per cent of copper and 20 per cent of zinc, and one containing 90 per cent of copper and 10 per cent of zinc. These latter three compositions are somewhat more expensive than the first one mentioned, and therefore, for normal needs, their use may be disregarded.

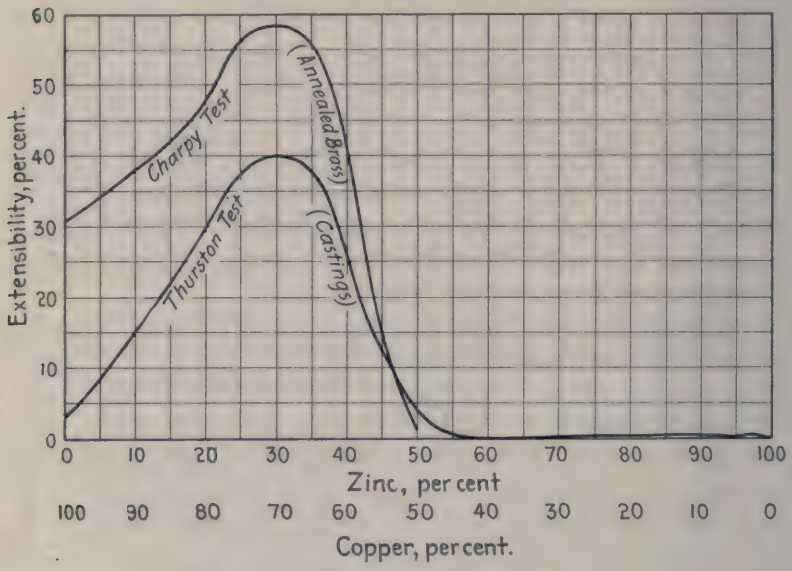


FIG 1.—Ductility of Copper-Zinc Alloys.

The first reason that the 70–30 mix is the most desirable is because such a proportion has the maximum ductility of all brass compositions. This is shown by Fig. 1.

It is realized that many tubes are made with less than 70 per cent of copper and more than 30 per cent of zinc. It is acknowledged, and shown in Fig. 2, that a tube made of brass with a copper content of 55 per cent and a zinc content of 45 per cent has a greater tensile strength than a tube composed of any other proportion of copper and zinc. It is felt, however, that the conditions to which a condenser tube are subjected

place a greater demand on ductility than on tensile strength. Therefore, that composition which produces maximum ductility and likewise which, very fortunately, has a high and ample tensile strength, is recommended.

Another reason for selecting tubes of the 70–30 mix is because such a composition gives a greater promise of a proper amount of cold working and annealing. Hot working is done more advantageously on brass tubes with a relatively high zinc content

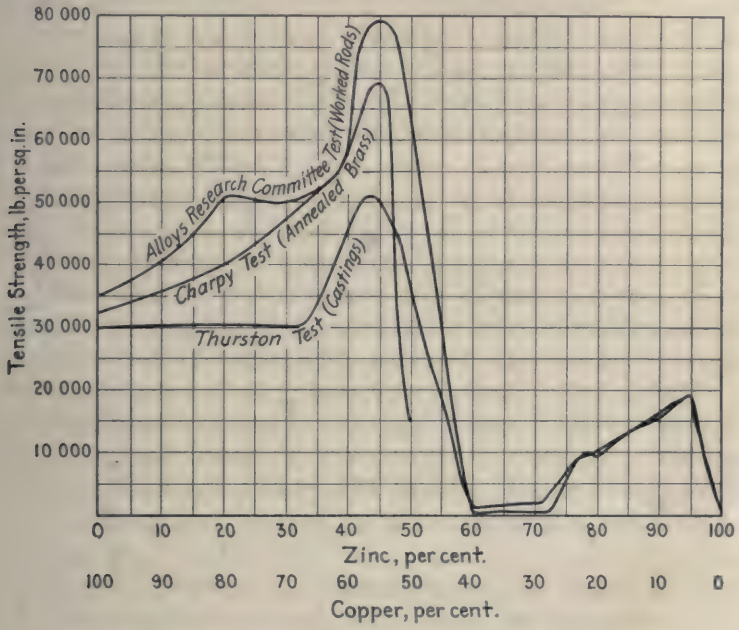


FIG. 2.—Tensile Strength of Copper-Zinc Alloys.

than on tubes with a lower zinc content. In other words, tubes containing from 45 to 37 per cent of zinc can be hot worked more easily than tubes with 30 per cent of zinc. There is always the temptation to work tubes hot as much as possible, because such an operation is less expensive than to work them cold. Therefore, in specifying a 70–30 mix, the very nature of the metal precludes to some extent the temptation to work hot. Cold drawing, when not too severe, assures a breaking down of the existing crystallization. This means both smaller and better

interlocked grains. Such work also makes annealing between each cold drawing or set of cold drawings a necessity. Both the cold drawing and annealing tend to greatly improve the quality of a tube. Thus such a composition offers a better assurance that a reasonable amount of cold drawing and annealing will be done than one with a higher zinc content.

Still a further reason for a 70–30 mix is that with such a composition the tube will have but one metallographic constituent. This constituent is a solid solution which may be considered as identical to a liquid solution which has frozen or become solid with absolutely no alteration in structure. Some engineers have feared that if a tube contained more than one solid solution—as would undoubtedly be the case if it contained less than 63 per cent of copper, and as might be the case if it contained from 63 to 67 per cent of copper—there would be danger of electrolytic action between the two constituents, which would result in a gradual destruction of the tube. The formula or composition recommended containing 70 per cent of copper and 30 per cent of zinc, tends to preclude the possibility of such electrolytic action since the tubes will contain but one constituent.

The reasons, therefore, for recommending a formula of 70 per cent of copper and 30 per cent of zinc for condenser tubes may be summarized as follows: (1) A greater demand is made on the ductility of a condenser tube than on its tensile strength, and a condenser tube possesses its greatest ductility with a copper content of 70 per cent and a zinc content of 30 per cent; (2) there is only a slight demand made on the tensile strength of the condenser tube, and a 70–30 mix has good strength, much greater even than is needed; (3) the purchaser is assured of better cold drawing and more careful annealing in a tube of a 70–30 mix than in a tube with a higher zinc content; and (4) such a tube contains but one constituent. Thus it is believed that with such a composition electrolytic difficulties, disintegration troubles, and splitting are removed to as great an extent as our knowledge of the matter at present permits.

CHEMICAL IMPURITIES.

It is also true that brass contains slight quantities of other metals, and as their presence has an important influence upon

the quality of the tube, it is therefore necessary to speak of them in some detail.

Lead.—High-grade condenser tubes should never contain more than 0.1 per cent of lead, or the ductility of the tube may be impaired. The lead does not alloy with the brass, but separates out in the form of globules between the crystals of the tube, a condition which necessarily weakens the tube. Lead is detected with the aid of a microscope, for it appears as black dots scattered at irregular distances over the surface.

Arsenic and Antimony.—Commercial copper usually contains arsenic and antimony as impurities. Their presence has an important influence on the quality of the tube produced, inasmuch as all of the arsenic and antimony in the raw copper is in turn found in the finished tube. The main objection to antimony is its tendency to cause cracks in the metal containing it. Sperry has found that as little as 0.02 per cent antimony, in a metal made up of 60 per cent of copper and 40 per cent of zinc, gives rise to incipient cracks during the annealing. Because of these conditions, it seems desirable to advise that the arsenic and antimony contents in brass condenser tubes should be kept under 0.02 and 0.002 per cent, respectively.

Bismuth.—Bismuth is sometimes found in tubes, and its effect is very similar to that of antimony, although, according to Sperry, it is less injurious. It is believed that 0.01 per cent of bismuth may be permitted in a tube without any detriment to it.

Iron.—Iron occurs as an impurity in both copper and zinc. It comes from the iron tools which are used in handling the metal, since small portions of it are dissolved when in direct contact with the molten alloy. It does not do any appreciable harm to the tube, providing it is not present to an extent greater than 0.075 per cent.

MECHANICAL TREATMENT.

The composition is only one of the conditions to be carefully studied in the manufacture of these tubes. The mechanical and heat treatments of the component materials have quite as much to do with the production of an acceptable tube.

It is impossible to say very much concerning the mechanical treatment which should be given a tube. It is very desirable that as much cold working as possible be done. Too much cold working should not be done between the annealings, however, for that would result in distorting the grain structure to so great an extent as to produce heavy slip bands, such that it would be difficult to completely eliminate them.

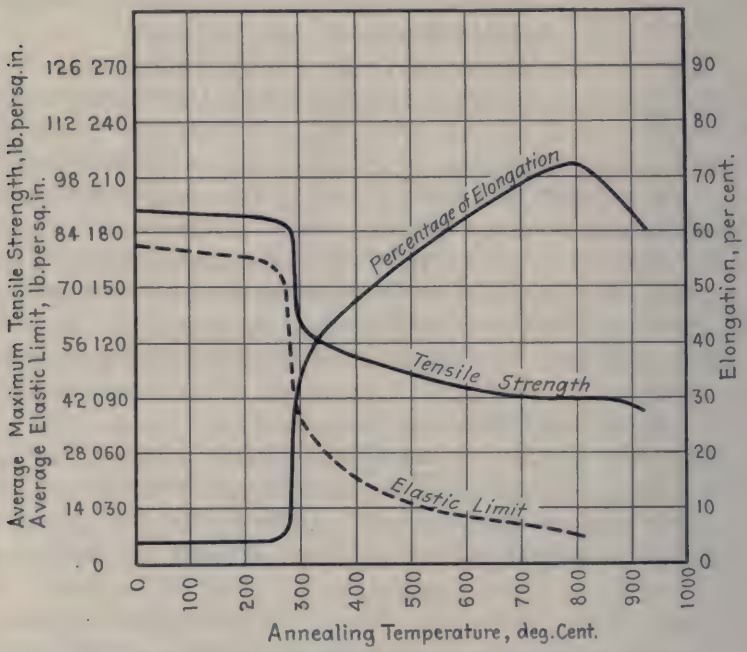


FIG. 3.—Influence of Heat Treatment on the Physical Properties of Brass (Copper, 67 per cent; Zinc, 33 per cent).

It is likewise more desirable to reduce the thickness of the tube by many light drafts rather than by a few heavy drafts. Such procedure guarantees more thorough and uniform kneading and interlocking of the tube grains.

A tube should be cold drawn, at the very least, six or seven times. Ten or twelve or even more cold drawings, though, would do no harm. In fact, the writer is of the opinion that the more cold drawings given a tube, provided that it is not drawn too

many times before it is annealed nor drawn with too heavy a draft at any one time, the better it will be.

HEAT TREATMENT.

The heat treatment should consist in annealing each tube between each cold drawing or set of cold drawings, at a temperature high enough to remove the grain distortion caused by the

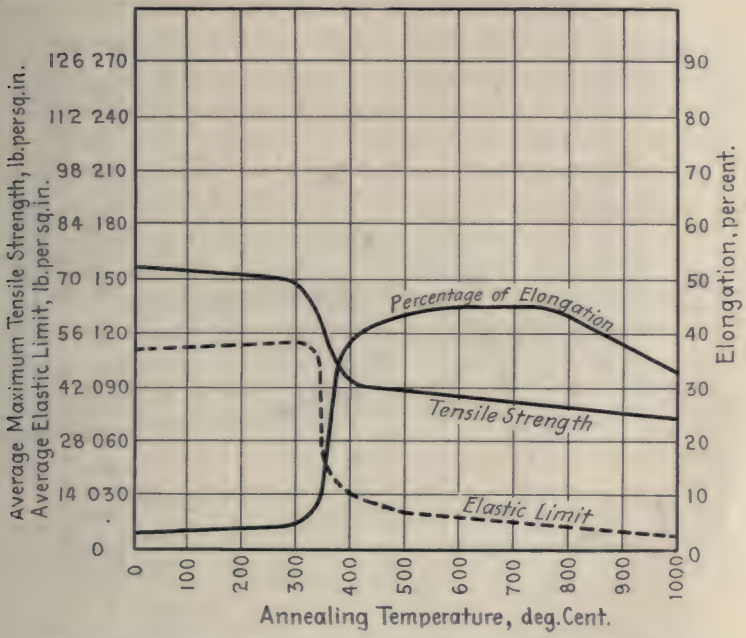


FIG. 4.—Influence of Heat Treatment on the Physical Properties of Brass (Copper, 90 per cent; Zinc, 10 per cent).

cold drawing, but not at a temperature high enough to permit crystallin growth to take place to such an extent as to cause coarse crystallization. After the final drawing each tube should be annealed just sufficiently to break down the distorted structure produced by the last drawing. Such a regulation of heat treatment should produce a tube of fine grain, and one which is relatively soft, and free from drawing strains.

No general specific temperatures can be given, as varia-

tions in the chemical composition and initial crystal size produce corresponding changes in the temperatures at which annealing commences. The correct annealing temperature of each composition should be ascertained through microscopical and physical-test researches, which should be performed for the sake of showing the influence of heat treatment on the grain size and on the physical properties of the tube. Referring to Figs. 3, 4,

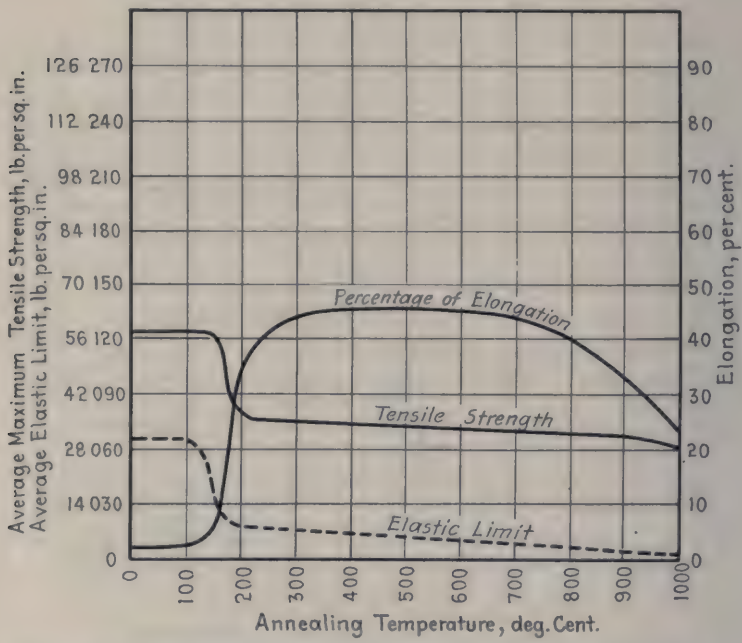


FIG. 5.—Influence of Heat Treatment on the Physical Properties of Electrolytic Copper.

and 5,[1] which show the influence of heat treatment on 67–33 brass, 90–10 brass, and electrolytic copper, respectively, it is probable that the annealing temperature in each of these cases which would tend to bring about the best results, would be that which is found where the curve denoting tensile strength crosses the curve denoting percentage of elongation.

[1] Produced from data furnished by Captain Grad, and appearing in a paper entitled "Laiton a Cartouches, Laiton a Balles, Cuivre Electrolytic," *Revue de Metallurgie*, 1909.

Length of time at the annealing temperature likewise plays a part. Hudson and Benough found that a Muntz metal, which is composed of 60 per cent of copper and 40 per cent of zinc, gave maximum ductility when heated for seven hours at a temperature between 560 and 690° C. The same metal when annealed but one-half hour required a temperature of 750° C. to bring about maximum ductility. In commercial practice it is believed that these temperatures are too high, for any one of them would tend to produce an unnecessarily large size of grain. The investigations of the writer showed that as tube metal was heated up to, but not held at, a given temperature, and cooled at different rates, as would be produced by cooling in the furnace, in air, or in ice brine, the metal which was cooled slowest—in other words was given the best opportunity to remain under annealing conditions—lost its original structure the earliest, and produced coarse crystallization the quickest.

This condition of affairs is splendidly illustrated by due reference to Fig. 6, which gives a set of photomicrographs illustrating the changes which heat treatment and rates of cooling produce in a condenser tube. The test specimens used in this experiment were all taken from one tube of the following composition:

	PER CENT.
Copper	66.94
Zinc	32.95
Iron	0.12
Lead	Trace
Antimony	None
Arsenic	Less than 0.02
Bismuth	" " 0.02

Thus, one is assured that the composition of each of these specimens is similar to the composition of each of the others, and that the structure of the metal in all cases before heat treatment was begun was the same. It is manifest that the tube undergoes no real change in structure until after a temperature of 300° C. is reached. The tubes heat treated at 400° C. show a breaking down of the preceding type of a somewhat distorted crystallization. The tubes heat treated at 600, 700 and 800° C. show that this crystallization grows as the temperature increases.

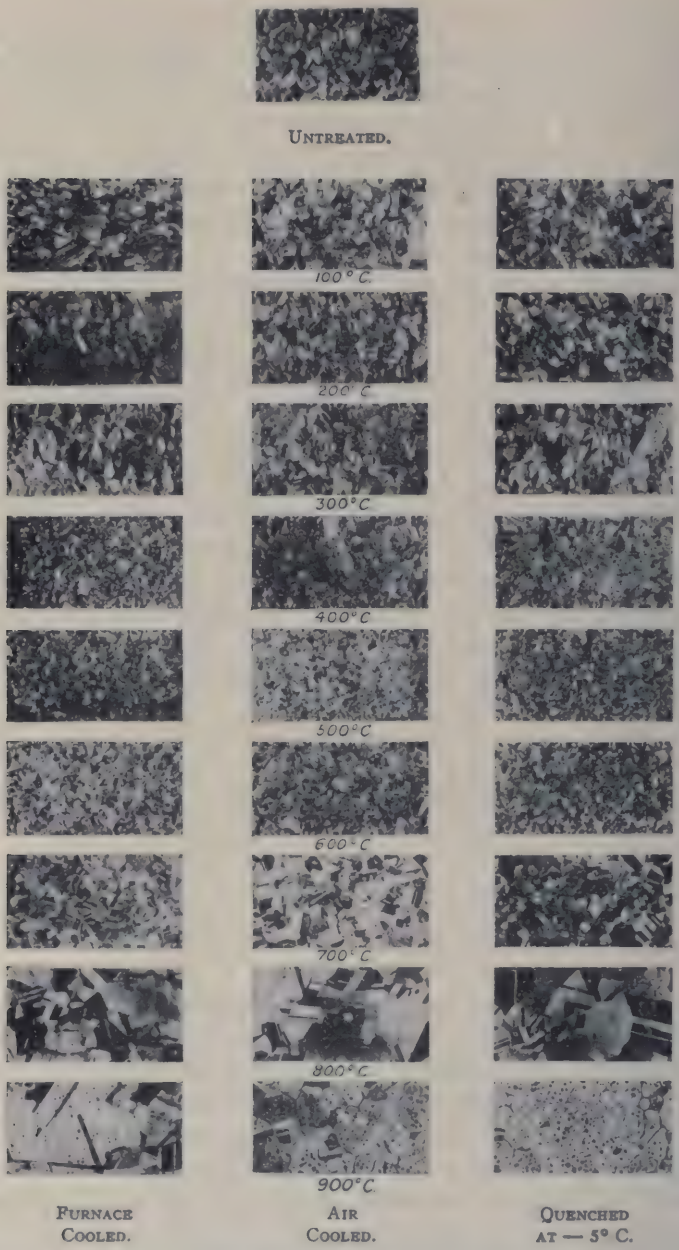


FIG. 6.—Influence of Heat Treatment on Brass (Copper, 67 per cent; Zinc, 33 per cent).

It is also evident that the longer the metal is held at an annealing temperature the coarser the grain, for the photomicrographs show that the earliest changes occur in those specimens which were the slowest cooled, or, in other words, were under the annealing influences the longest. The specimens air cooled and quenched at 900° C. show characteristic burned crystals.

All of these structures were brought out with a special etching reagent consisting of hydrogen peroxide and ammonia. They were all initially magnified 75 diameters, but for reproduction here have been reduced to 20 diameters. They have but one component, namely, alpha crystals, because their composition is such that there is present only an alpha solid solution.

SPECIFICATIONS.

The significance of the appended specifications for the purchase of condenser tubes which conform more or less closely to the specifications which have been prepared by the U. S. Navy Department, may now be appreciated. The essential details in the Navy Department's specifications have been adopted, and, in addition, it has seemed best to add a section requiring microstructural examination. The writer feels that such a method will enable one to determine beyond any question whether the particular tube examined has been properly treated, and to infer by analogy whether the treatment for the entire lot has been of a satisfactory character.

APPENDIX.

SPECIFICATIONS FOR BRASS CONDENSER TUBES.

1. *Material.*—Tubes should be made from lake or electrolytic copper, and high-grade spelter. No scrap may be used in their manufacture, except such clean scrap as may result from process of manufacture of similar articles. The order for the tubes will state the composition.

2. *Visual Inspection.*—For purposes of inspection, the tubes will be divided into lots of 600 lb. plus 1 per cent for test purposes. The latter will be exclusive of the number required on the order, and will be furnished at the manufacturer's expense.

"The tubes for each lot will be assembled for inspection in a well-lighted place, and so arranged as to simplify inspection as much as practicable. Tubes that do not show uniform annealing under handling will be rejected from the lot.

"All tubes must be seamless, true to form, of an equal thickness throughout, of workmanlike finish, free from cracks, seams, and other defects, and stiff enough to lie straight when resting on supports 6 ft. apart.

"Each tube showing slivers of 'spilly' metal, either during process of manufacture or upon final inspection, will be rejected."[1]

3. *Condemnation.*—If 10 per cent of the tubes on final inspection of any lot show any of the above defects, the lot will be rejected.

4. *Chemical Analysis.*—"Chemical analysis must show the material to be in accordance with the following table:"[1]

Copper	As specified
Zinc	As specified
Tin	None unless specified
Lead	Under 0.1 per cent
Iron	Under 0.075 "
Antimony	Under 0.002 "
Arsenic	Under 0.02 "
Bismuth	Under 0.01 "

[1] Navy Department Specifications, 44–T–7.

Cuttings for analysis must be taken by a milling machine, or such other method as will insure clean chips, the size of a medium pin head, and they must be from equal portions of each of the test tubes representing a test lot. "The whole amount taken from the tubes must not be less than 8 oz."[1]

These cuttings for analysis must be thoroughly blended and then one-half of the amount must be given to the manufacturer and the other half retained by the inspector, the amount in each case being enough for three analyses.

5. *Rejection.*—If the first analysis shows a failure, the material is condemned. "If the manufacturer does not accept the rejection on the first analysis, a second (check) analysis

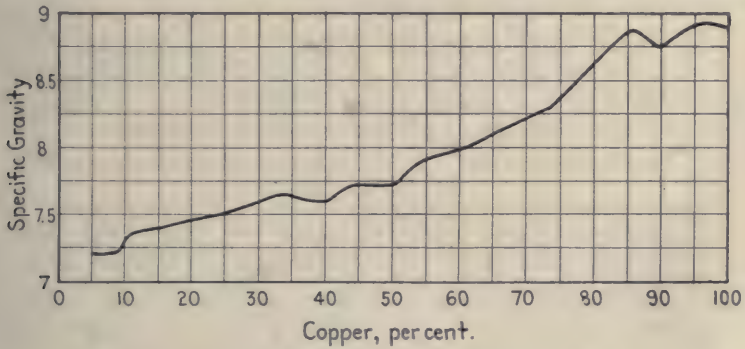


FIG. 7.—Specific Gravity of Copper-Zinc Alloys.

will be made. If this also shows a failure because of close agreement with the first analysis, the material stands rejected.

"If the second (check) analysis, however, is at variance with the first analysis, then a third analysis will be made. In this case, if the last two analyses made show agreement and are satisfactory, the tubes will be accepted; otherwise they stand rejected."[1]

WEIGHT.

6. *Weight.*—"The weight of the test tubes of any lot will be used in calculating the weight of the lot.

"All tubes must be of the required gage on the thinnest side."[1]

[1] Navy Department Specifications, 44-T-7.

The weight of the tubes will be figured in pounds per cubic inch of metal, and the correct figure for this weight will be determined from Fig. 7,[1] which shows the variation of the specific gravity of the metal with the variation of the copper and zinc contents of the brass.

"An excess of weight of not more than 5 per cent will be allowed on all tubes."[2]

ANNEALING AND MICROSTRUCTURE.

7. *Annealing and Microstructure.*—All tubes must be annealed and acid-cleaned. Microsections of the tubes at magnifications of 75 diameters must show no strains, and no grain larger than $\frac{1}{4}$ in. in diameter.

8. *Rejection.*—If a microsection shows indications of strains or if a grain larger than $\frac{1}{4}$ in. in diameter be observed, and if an examination of a second group of the same lot of metal displays the same tendency, the tubes will be rejected.

PHYSICAL TESTS.

9. *Hammer Test.*—Test tubes will be hammered with a hand hammer until flat. The tubes must not show signs of strain under this test. "The term 'hammered flat' will be interpreted as follows: A micrometer caliper set at three times the thickness must pass over the tube freely throughout the flattened part, except at the point where change in element of flattening takes place."[2]

10. *Expanding Test.*—"Each test tube will be subjected to an expanding test. This will be carried out by driving a pin with a taper of $1\frac{1}{2}$ in. to the foot into the tube, until the internal diameter has been increased $16\frac{2}{3}$ per cent."[2] The tube must not split during this test.

11. *Pressure Test.*—"All tubes will be subjected to an internal pneumatic pressure of at least 80 lb. per sq. in., the tubes being immersed in water during applications of the latter pressure."[2]

12. *Rejection.*—"The failure of more than one test tube to stand each test or the same tube failing in more than one

[1] Values taken from *Alloys*, by A. H. Sexton.

[2] Navy Department Specifications, 44-T-7.

test, will cause the rejection of the lot."[1] In the case of the failure of one tube under a single test, two other tubes must be subjected to the test, and the failure of one of these will cause the rejection of the lot.

PACKING AND MARKING.

13. *Packing.*—The tubes must be packed solid enough to stand transportation and handling without injury.

14. *Case Markings.*—Each case must have stenciled on it the number and dimensions of the tubes, the composition, the name of the manufacturer, and the company and plant to which the tubes are being shipped.

15. *Ferrules.*—The ferrules will be of the same material as the tubes.

[1] Navy Department Specifications, 44-T-7.

DISCUSSION.

Mr. Webster.

MR. W. REUBEN WEBSTER (*presented in written form and read by the author*).—The subject with which this paper deals is one of very great importance. The investigation which forms its subject matter should, however, be understood as relating to a minor difficulty sometimes encountered in connection with the employment of condenser tubes and not considered as dealing with the major problem with which both makers and users of such tubes are confronted, namely, the cause and the remedy for the serious corrosion which occurs when such tubes are used in contact with sea water.

As restricted, however, as is the phase of the subject covered by the investigation, namely the failure from cracking or splitting in service sometimes experienced, it is open to some criticism. On account of the fact that the literature of the non-ferrous metals and alloys is full of statements which are liable to mislead, either because of incorrectness or because of changed conditions which render them no longer of consequence, it is important that a paper of this character should be subjected to careful scrutiny in order to determine the reliability of its conclusions.

In that it calls attention to a means of securing tubes which will not fail from the well-known phenomenon of age or season cracking, it will be of value, although such tubes have been procurable for many years and have been demanded by the specifications of the Bureau of Steam Engineering for over fifteen years. The clause in the latter requiring annealing as a final process was originally adopted on the recommendation of the speaker fifteen or eighteen years ago, and was intended solely as a preventative of cracking. Since then, millions of pounds of condenser tubes have been made without a single reported failure from this cause. Certain statements are, moreover, made and certain conclusions drawn which are unwarranted by any established or recognized facts. Many attempts have been made by engineers unfamiliar with the art of tube making to draw specifications, some of which have been un-

necessarily restrictive and others which have almost inevitably required a product which was certain to give trouble from cracking. Mr. Webster.

In discussing specifications for a material, it is necessary to consider the facts which relate to ease and cheapness of fabrication, as well as to satisfactory performance in service. Any restrictions imposed contrary to established and accepted practice should be clearly proved as necessary.

This does not seem to have been entirely the case, as will be pointed out later.

It is stated that "it is believed for several reasons that for fresh unpolluted water the best service can be obtained from condenser tubes containing 70 per cent of copper and 30 per cent of zinc." No sufficient reasons are given, however, nor any facts established in support of such reasons. Large quantities of condenser tubes of mixtures other than the one given have been used in fresh unpolluted water with entire success and satisfaction. It is entirely possible to make tubes for such service of other mixtures which will last indefinitely and show no trouble whatever from corrosion or splitting.

The reasons given for the choice of the 70–30 mixture, namely, its maximum ductility, are not entirely relevant in that no connection is shown between lack of ductility as such and service failure. Similarly no connection is shown between hot working and lack of any desirable quality. It would, in fact, be difficult to refute the statement that a tube made entirely by hot working would give satisfactory service.

With respect to the impurities discussed, the lead content is unduly restrictive. Spelter which will allow a lead content of 0.1 per cent in 70-30 brass can only be obtained at a premium. No advantage over brass containing 0.2 or 0.3 per cent can be shown which will warrant the premium. Antimony in minute quantities interferes seriously with the cold working of brass, but such is not the case with arsenic even in considerable quantities. There is no evidence presented, and it is doubtful if any is available, to show that bismuth in a finished tube is deleterious. The same is true of iron, although in quantities much in excess of the figure given it hampers the operations of the tube maker.

Mr. Webster. The statements concerning the effect of mechanical treatment are unspecific and in some cases incorrect. It is inferred that several light drafts in succession are more advantageous than one heavy draft having a magnitude equivalent to the total of the lighter ones. No evidence of this is presented; it is doubtful if it is so and established practice is contrary to it. The same is true with respect to the number of drafts. Experiment has established that two or three drafts are sufficient to attain the maximum physical properties of the metal.

Turning now to the matter of heat treatment, it is stated "it should consist in annealing . . . at a temperature high enough to remove the grain distortion caused by the cold drawing, but not at a temperature high enough to permit crystallin growth to take place to such an extent as to cause coarse crystallization." The requirements of practice manifestly demand that intermediate annealings should take place at a temperature which will produce maximum ductility and the crystal size corresponding to that temperature could scarcely be defined as anything but coarse. That such practice is detrimental to the performance of the tube in service is nowhere demonstrated.

With respect to the recommended finished annealing temperature, no evidence is presented to show that a tube annealed at the recommended degree is superior to one annealed at a temperature several hundred degrees higher.

Considering now the proposed specifications, the high-grade spelter stipulated is unnecessarily expensive for the requirements of the service to which these specifications apply. The requirement that "all tubes must be of an equal thickness throughout" cannot in general be met. Such tubes usually show an extreme variation in thickness of at least 10 per cent. Much doubt exists as to the necessity of the requirement that "each tube showing slivers of 'spilly' metal . . . will be rejected." Some experiments under laboratory conditions indicate that surface imperfections tend to serve as foci for corrosion, but extended observation of the behavior of tubes under service conditions does not confirm this. The chemical requirements have already been discussed. The specifications of the Navy Department

do not impose these restrictions, although it might be so inferred from the foot note. Mr. Webster.

A serious error exists in Section 6 of the specifications, wherein a curve is given from which the weight of the tube is to be figured. The values given by the curve are incorrect. Determinations made by the Bureau of Standards for the purpose of checking this curve gave 8.465 for the specific gravity of a 66-34 tube and 8.543 for Admiralty mixture, values over 4 per cent in excess of those given by the curve. In view of the fact that the 5 per cent excess in weight allowed by the curve becomes less than 1 per cent in fact, this is a serious error. The specifications earlier required that all tubes must be of the required gage on the thinnest side. In order to provide a tolerance for the variation in gage which is inevitable, it is the practice to meet such a requirement by making the tubes over weight and consequently over gage. It is manifest that if, because of the error of this curve, the specific gravity of the tube is figured over 4 per cent too low, the allowance for gage variation becomes less than 1 per cent. It is desired to call particular attention to this provision as indicating the manner in which the acceptance of published statements without verification may lead to serious embarrassment.

Under "Annealing and Microstructure" it is required that "microstructure . . . must show no grain larger than $\frac{1}{4}$ in." It has already been pointed out that such a restriction is of doubtful value. In addition, an individual crystal frequently occurs of much greater size than the average of those by which it is surrounded. If a limitation of crystal size were demonstrated to be necessary, it would appear to be more scientific to define it in terms of average size.

In conclusion it is desired to expressly deny any wish to indulge in harsh or destructive criticism of this paper. It is felt, however, that in the interests of scientific accuracy, many of its statements must be called in question. The literature of the non-ferrous metals is already full of erroneous information, which is passed on from hand to hand and gives rise to serious mistakes and misunderstanding. Great need exists for investigation in this field under rigid scientific procedure in order that a basis of undisputed facts can be established. How large a task

Mr. Webster. this is can readily be understood from the magnitude of the work already done by the Corrosion Committee of the Institute of Metals.

Mr. Price. MR. W. B. PRICE (*presented in written form and read by the author*).—Mr. White makes the following statement in his paper: "It is likewise more desirable to reduce the thickness of the tube by many light drafts rather than by a few heavy drafts. Such procedure guarantees more thorough and uniform kneading and interlocking of the tube grains." This is contrary to fact.

It is fairly well known in the brass industry, and has been shown by Mathewson and Phillips, that the recystallization of brass, especially at low temperatures, is largely dependent upon a sufficient reduction or deformation of the metal before annealing. Metal which has received a relatively slight reduction by rolling is in the aggregate more coarsely crystallin after annealing than the metal which has received a relatively high reduction. There seems to be a rather sharp line between the coarser metal and the finer metal. This occurs at a reduction of 3 numbers on the Brown & Sharpe gage, or a reduction of slightly under 30 per cent. Below this value the metal is not only coarsely crystallin in the aggregate, but highly non-uniform. Above this value the crystallin grain is quite uniform. In the absence of exact knowledge concerning the recrystallization process it is not possible to give a thoroughly satisfactory explanation of these facts. It has, however, been demonstrated that annealing after local deformation produces great variation in the size of the recrystallized units. The smaller crystals are located at the seat of the greatest deformation while larger crystals have grown out into the slightly deformed or unaltered metal. In this case it is probable that in the metal rolled below 3 numbers there are many different zones of deformation in which some crystals are only slightly overstrained, while others are considerably distorted. Under these conditions we would expect rapid growth of large crystals from one zone into another, while where the deformation was comparable throughout a considerably extended region the crystals would be more uniform in size.

Relative to the effect of annealing on rather hard material, it has been shown by Grard and others that at moderately high temperatures the amount of reduction has very little effect on

the ultimate results. Where the annealing temperature is very low, the rate of growth of new crystals is highly dependent upon the temper, even in hard metal. It is probable that the rate of growth of crystals in overstrained metal is dependent on the number of units or nuclei capable of growth. Where these are innumerable, as is probably the case in metal rolled above 3 or 4 numbers, the growth and ultimate size attained is comparable, whatever the reduction; but where these are relatively small in number, as in the reductions below 3 numbers, the effects of local deformation are seen, some nuclei growing much larger than others and some crystals remaining sensibly unaltered. Mr. Price.

Referring to Figs. 3, 4 and 5, Mr. White states that "it is probable that the annealing temperature in each of these cases which would tend to bring about the best results, would be that which is found where the curve denoting tensile strength crosses the curve denoting percentage of elongation." Taking Fig. 3 for 67–33 brass, which Mr. White gives as the nearest alloy to 70–30 brass, this point would be in the neighborhood of 325° C.

At this low temperature complete recrystallization would not take place, and in order to obtain a uniform grain size of $\frac{1}{4}$ in. diameter at a magnification of 75 diameters, it would be necessary to anneal at a temperature in the neighborhood of 500° C. after the metal had had a reduction of not less than 30 per cent.

MR. CLOYD M. CHAPMAN.—It is very interesting to the users of condenser tubes to hear the speaker on one side say that it is perfectly easy to make good tubes even without following any very restricted specifications, and the speaker on the other side say that by following a rather rigid specification he reduced his troubles to almost zero. I know the difficulty we have had in the past of getting condenser tubes that would stand up in service, and of the large number that had to be removed; the frequency and difficulty of making the removals and the consequent shutdowns of the condensers necessary. If it is so easy to make a perfectly good tube which will stand up to the work, I cannot understand why we don't get them. Why are not all tubes well made? I should like an explanation. If it is not necessary to do these things which it has been shown, Mr. Chapman.

Mr. Chapman. in one case at least, removed some of the troubles the steam engineer has to contend with, why don't we get the good tubes?

I should also like to ask these gentlemen for an explanation of pin holes in condenser tubes.

Mr. Devries. MR. R. P. DEVRIES.—I should like to ask Mr. White whether Section 8 of the specifications appended to his paper, relating to annealing and microstructure, was inserted in the specification of the Navy Department as the result of this work.

Mr. White. MR. A. E. WHITE.—If one will read over the specifications carefully it will be noted that such sections as have been taken directly from the specifications of the Navy Department have been headed by quotation marks. The specifications relative to microstructure and rejection, Sections 7 and 8, are not, so far as I know, in the government specifications.

Mr. Devries. MR. DEVRIES.—Then I should like to ask this further question: What is really meant, in Section 8, by the expression "If a microsection shows indications of strains?" And also, in Section 7, the statement appears: "Microsections of the tubes at magnifications of 75 diameters must show no strains." This, to me, is an entirely new idea. How do you detect strains, apart from grain size, by use of the microscope?

Mr. White. MR. WHITE.—Our method of detecting strains—and it seems to work out rather satisfactorily—is to take a piece of metal, polish it very carefully so there will be absolutely no indication of scratches even at high magnifications, and etch it in a mixture consisting of suitable proportions of ammonia and hydrogen peroxide. If across the surface of the crystals are found parallel bands such as are noted in practically all of the photomicrographs taken from samples heated under 400° C., we feel that the metal is in a state of strain.

The magnification of 75 diameters was selected because in one of the larger manufacturing plants with which I am in touch that is a standard magnification. The $\frac{1}{4}$-in. grain size requirement was introduced because, at that magnification, if the grains are larger than that it indicates a crystal size too coarse to be satisfactory. With such a large grain size there would be a tendency—and we are trying to remove these tendencies—for corrosion, since it is generally admitted, from a large number of recent researches, that the coarser the grain the greater the tendency toward corrosion.

MR. WEBSTER.—In response to the question as to why there is so much difficulty with condenser tubes failing in service, I should like to call attention to the fact that this paper deals almost entirely with the subject of cracking, and not with the subject of corrosion. In my previous discussion I called attention to that fact. The causes of corrosion which produces pin holes have been extensively investigated, particularly by the Corrosion Committee of the Institute of Metals of Great Britain. That work has already gone into several hundred pages, and is as yet by no means complete. The paper under discussion particularly deals with the endurance of condenser tubes in fresh, unpolluted water, under which conditions the formation of pin holes does not occur. **Mr. Webster.**

MR. CHAPMAN.—Am I to understand, then, that the formation of pin holes is due to polluted water, or to corrosion caused by constituents of the water, rather than to any peculiarity or irregularity of the tube at the point of failure? **Mr. Chapman.**

MR. WEBSTER.—All the evidence points to that fact. The pin holes are due to what is known as selective corrosion. When a brass condenser tube is put in service in contact with sea-water, the zinc immediately begins to dissolve out. In general, this action takes place quite uniformly and may proceed without interfering with the serviceability of the tube until the zinc is very thoroughly removed from the brass and only the copper left. As a result of this action, the strength of the brass is gradually destroyed. It frequently happens, however, that the removal of the zinc localizes in small areas which are clearly defined, and which rapidly penetrate clear through the wall of the tube. This sometimes takes place with great rapidity, only a few weeks being necessary to produce perforation. The reasons for the erratic action are not as yet well understood. **Mr. Webster.**

MR. WHITE.—Mr. Price takes exception to my statement: "It is likewise more desirable to reduce the thickness of the tube by many light drafts rather than by a few heavy drafts." I admit that this part of the statement is not specific; in fact, I thought the manufacturers might object if I were inclined to make it specific. In the steel industry they have passed through a stage where it is realized that it is better to develop a proper grain size by a large number of drafts of light intensity rather **Mr. White.**

Mr. White. than by a few drafts of heavy intensity. I think the Bethlehem Steel Co. have scrapped a large 125-ton hammer within the past few years partly because of that feeling. It is realized that if the reductions are made with too light a draft there will then result surface or skin hardening. This is an unfortunate condition. It is felt, however, that the draft or reduction which should be given a metal should only be just heavy enough to work the metal all the way through, and that this should be repeated as many times before annealing as the metal will stand, rather than to resort to the use of but one absolute reduction between the anrfealings. I appreciate that there are two points of view on this matter, and I can feel the weight of the opinions of those who think that but one reduction is desirable. I also realize that this is a matter which merits further investigation to determine which of the two ideas is the more correct.

Mr. Price also calls attention to the statement referring to Figs. 3, 4 and 5, which show the influence of heat treatment on 67–33 brass, etc., stating that the annealing temperatures were not correctly given. I ask Mr. Price merely to note the fact that I said that "it is probable." I could not say definitely that the temperature must be 325° or 350° C., etc., because it is appreciated that differences in the degree of mechanical working and differences in the chemical composition are going to cause this temperature to fluctuate—either to lower values or to higher values. The real temperature criterion was taken care of, I believe, in the photomicrographs shown. They indicated an insufficient annealing at 300° C., but a perfectly satisfactory annealing at 400° C., since the slip bands had disappeared and the desired recrystallization had taken place.

I do not desire to get into a controversy at this time with Mr. Webster on certain of the matters to which he has taken exception, because such a discussion might tend possibly to be one between consuming interests and producing interests. I should like, however, to say that it is with peculiar interest that Mr. Webster's discussion has been heard; it is felt, however, that he has unfortunately misinterpreted the object of the paper, which was to make public the experience of a large consumer of tubes, with a specification which has been proving eminently satisfactory.

Mr. White.

It is not anticipated that the tentative specifications presented will stand the scrutiny of time without some revision. They were given because, up to June, 1913, the firm for whom they were prepared had failed to find any relief from the troubles posing under the name of "season cracking." No tubes purchased since the adoption of these specifications, several years ago, have caused trouble.

It is admitted that a certain number of manufacturers and consumers of tubes have been aware that brass, when in a state of strain, is subject to "season cracking." This knowledge however, has not been as general as one might expect from the nature of Mr. Webster's remarks, and because of the limited appreciation of the phenomenon it was felt that the paper would be of value. It is true that "millions of pounds of tubes have been made without a single reported failure." On the other hand any consumer will advise that, because of the trouble he has encountered from this source within the past fifteen years, he has been put, expressing it very mildly, to considerable inconvenience. The specifications given, it is felt, will be of value in helping to eliminate this cause of failure.

Mr. Webster brings forth a number of counts in which he disagrees with the statements of the author. These will be discussed individually.

1. First of all he states that no sufficient reason is given for recommending a mixture of 70 per cent of copper and 30 per cent of zinc. In the paper[1] are given four reasons why such a mixture is recommended. The fourth reason, the production of a material with but one constituent, is sufficiently strong without the addition of any others. With less copper there might result, and in most cases would result, a product with more than one constituent. From the corrosion standpoint such a condition is bad. With more copper, at least up to 95 per cent, no trouble should be encountered. Tubes with these higher copper contents were not recommended, however, solely because of their higher price.

2. Concerning impurities, it is admitted in the case of spelter that when containing less than 0.1 per cent lead it is secured at a premium. The premium has little influence on the ultimate

[1] P. 156.—ED.

Mr. White. cost of the tube, however, and therefore such criticism is unjustifiable. Further, lead in quantities greater than 0.3 per cent greatly accelerates the rate of corrosion. Smaller quantities play their part although not as noticeably. For these reasons, therefore, the lead should be kept under 0.1 per cent in the finished tube.

The mines producing arsenical copper formerly sold a poor-grade product. To eliminate the possibility of the use of such material the arsenic specification was introduced. It is admitted that in due time this requirement will become obsolete.

Very little bismuth is ever found in tubes; in fact most of the copper and zinc used in tube manufacture is free of bismuth. According to Sperry its action in brass is similar to that of antimony and therefore its permissible amount should be limited.

The introduction of too much iron means either the use of scrap or faulty shop methods. Either of these conditions is objectionable. No note need be made of its direct effect on the quality of the material, for either of the above two factors is serious enough and is sufficient reason why its amount should be limited.

3. The same remarks made in answer to Mr. Price's criticism concerning the effect of mechanical treatment have equal force with reference to Mr. Webster's criticism. It is the writer's opinion that the more drafts given a metal between the annealings—provided each draft is of sufficient intensity to work the metal all the way through—the better will be the quality of the final product. Experience has taught that this is true in the steel industry, and the author will believe it holds equally true in the non-ferrous industry until presented with actual proof to the contrary.

4. Mr. Webster states: "Maximum ductility and the crystal size corresponding to that temperature could scarcely be defined as anything but coarse." All true, but why is *maximum ductility* necessary? *Sufficient ductility* is secured after the complete elimination of the strains and that, too, without the sacrifice of fine grains.

5. Concerning the "finished annealing," Mr. Webster is most unspecific. In speaking of "at a temperature several hundred degrees higher" does he mean the Centigrade or Fahrenheit system? If according to the Centigrade system

several hundred degrees higher would tend to ruin his material because it would be altogether too soft. If according to the Fahrenheit system it would result, if not in too soft a material, then in the development of such coarse crystals as to make the metal readily subject to corrosion. Mr. White.

6. The "equal thickness throughout" section is taken directly from the Navy Department specifications. There has been no trouble in the past in properly interpreting it and the writer sees no reason why trouble should be encountered in the future.

7. In spite of what Mr. Webster might lead one to infer regarding the foolishness of the requirement that "each tube showing slivers of 'spilly' metal will be rejected," it is true that consumers are more and more rigidly enforcing this section of the specifications. The writer does not know how rigidly the Navy Department enforces this section in the mill with which Mr. Webster is connected. He does know, however, that in one large mill the Navy Department does most emphatically impose this particular restriction.

8. Mr. Webster's criticism relative to the use of the diagram in Fig. 7 is appreciated. The diagram, however, was inserted more as a suggestion relative to the way of getting at the theoretical weight of tube metal than as a diagram for actual service. The values in the diagram are slightly too low for tube metal because they were secured from cast metal, while the actual values in question should be for rolled or drawn metal.

9. Mr. Webster's suggestion relative to the proper way for determining grain size may be as scientific as the way designated in the article, but the writer does not think it "more scientific." The author purposely refrained from adopting such a proposed method because it is much more cumbersome, does not tell one any more, and is not nearly as rapid as the test in the specifications.

After all, coming down to the final issue, is not the real criterion of specifications a determination as to whether or not material purchased under their requirements is satisfactory? Can there be any better test of the value of specifications than service? Is not a service test of some years good evidence that the specifications are fulfilling their function, and satisfactorily meeting the requirements demanded from them?

A NEW FORM OF SPECIFICATIONS FOR CONCRETE AGGREGATES.

By Cloyd M. Chapman.

SUMMARY.

This paper proposes that instead of specifying that concrete aggregates, particularly sand, be of a certain fixed minimum standard of quality, which may not be attained in certain localities, the specification be made to read, in effect, as follows:

"The materials used shall be of such quality, and shall be used in such proportions, as to produce a concrete which shall show a compressive strength of 2500 (or 2000 or 1500) lb. per sq. in. at the age of 28 days, when tested in accordance with the standard methods of testing."

The paper further gives a brief outline of the manner in which this form of specification is utilized in practice by Westinghouse Church Kerr and Co., and their method of checking up the quality of fine aggregates as they are received on the job, by the use of the sand tester.

A NEW FORM OF SPECIFICATIONS FOR CONCRETE AGGREGATES.

By Cloyd M. Chapman.

The difficulties that surround the writing of specifications are sufficiently numerous and perplexing with almost any class of materials, but they are particularly so with concrete aggregates. The reason is that the materials themselves vary so greatly that no universal standard is applicable to them. They are so bulky, heavy and of such low value that it usually is not profitable to ship high-grade materials into districts not possessing them.

To be generally acceptable specifications for econcret aggregates should fulfill two requirements, namely: (1) they should insure the production of suitable concrete if the aggregates are properly used; and (2), they should permit the use of materials found in the vicinity of the work, if such materials are capable of producing concrete of the required quality.

The present method of specifying may insure the quality of the material, but it does not permit the use of a wide choice of aggregates from which first-class concrete may be made.

It is quite possible to write specifications for sand and stone which will answer the purpose in New York, others for Pittsburgh, and still others for New Orleans, but they will all differ from each other if they are to be met by the local products.

Concrete is a cheap building material because it is composed largely of inexpensive aggregates, and for economic reasons these aggregates should be secured from deposits in the vicinity of the point of use. Whatever materials are locally available for aggregates must be used in the great majority of cases, for aggregates which must be transported long distances are no longer inexpensive. The material at hand capable of making concrete of fair quality will generally be used in preference to a better material which must be brought from a distance at considerable cost for transportation.

It is generally true that even a very poor sand, that is, one which compares very unfavorably with standard Ottawa sand when tested as prescribed, show a tensile strength approxi- compressive strength if sufficient cement is used. Take, for example, a fine, poorly graded sand, which in a 1 : 3 mortar gives a compressive strength equal to only 60 per cent of that obtained with standard Ottawa sand. Such a sand used in the proportion of 1 : 2 or 1 : $2\frac{1}{4}$ will usually give strengths fully equal to the 1 : 3 mortar made of Ottawa sand. When the only available sand is of such a quality, it is necessary to increase the proportion of cement until the required strength is obtained.

Our present specifications for sand require that it shall, when tested as prescribed, show a tensile strength approxi- mately equal to that obtained with standard Ottawa sand. A rigid adherence to these specifications would mean that in many sections of the country no concrete work could be done without importing aggregates from a distance. The result is that the specifications are ignored in those localities and the work done without them.

On the other hand, where most excellent materials are available, the present specifications for sand do not permit of a variation of the proportions used no matter how good the materials may be. For instance, some well-graded sands show strengths 40 per cent higher than that obtained with Ottawa sand in 1 : 3 mortar, and in 1 : $3\frac{1}{2}$ or even 1 : 4 mortar show strengths equal to that obtained with Ottawa sand in the pro- portion of 1 : 3. Yet this sand receives no credit for quality under the present form of specifications.

Specifications serve their purpose when they secure the products described, but they serve the industry best when broad enough in scope to cover all the materials capable of producing a product of the desired quality. To insist that throughout this country no sand may be used in concrete which does not show a compressive strength approximately equal to that of a standard sand when tested in 1 : 3 mortar, is in a way analagous to insisting that all structural steel shall be made from an ore containing a certain minimum percentage of iron.

In order to cover and include all materials which are capable of producing concrete of the quality required for the

particular service it is to perform it is only necessary to specify the result required instead of specifying the materials used.

Let it be assumed, for instance, that for mass foundations and footings, for retaining walls and abutments, and for similar service, a concrete which has a compressive strength of 1500 lb. per sq. in. at the age of 28 days is suitable. For reinforced walls and matts where stresses are moderate, a compressive strength of 2000 lb. per sq. in. at 28 days may be required, while for reinforced floor slabs, beams, girders columns, etc., a minimum compressive strength of 2500 lb. per sq. in. at the same age may be necessary.

By specifying the results required and permitting the use of such materials as will produce these results when tested under specified and standardized conditions, it is possible not only to properly safeguard the product but to permit the use of such materials as are available in each locality.

It is also true that in many cases the local materials are of such poor quality and would require such a large proportion of cement to fulfill the specifications, that it would be economical to bring in better material even from a considerable distance, the saving in cement paying the freight. Specifications of this kind might take some such form as the following, in which all figures are purely arbitrary and in no sense proposed as standard:

"The materials used shall be of such quality, and shall be used in such proportions as to produce a concrete which shall show a compressive strength of 2500 (or 2000 or 1500) lb. per sq. in. at the age of 28 days, when tested in accordance with the standard methods of testing."

This form of specification is obviously susceptible to modification to cover varying conditions and qualities. For instance, to insure against a material which sets or hardens slowly, and consequently requires forms being kept in place an unusual length of time, the specifications may require a certain minimum strength to be attained in three days. When compressive strength is not the prime requisite, as for instance, in sea wall or tunnel work, the requirements as to impermeability or density may be inserted, either in place of, or in addition to, the strength requirement.

It would probably be desirable to add some further qualify-

ing clauses to the specifications, such as the limit of size of particles, the character of the materials composing the aggregates, freedom from constituents liable to cause deterioration, etc.

The standard methods of testing referred to in these specifications would have to be very carefully worked out, as the value of the specifications is largely dependent upon the reliability of the test, just as is the case in almost all specifications of a like nature which are dependent upon tests.

The method at present most commonly employed, except in the cases of some of the railroads and on government work where aggregates, particularly the sand, are systematically tested, is to practically ignore the quality of materials, except the cement, and to arbitrarily specify proportions that will give good enough results with almost any aggregates. By requiring that all concrete in a reinforced structure shall be made in the proportions of say, 1 : 1½ : 3 or 1 : 2 : 4, we are simply "playing safe." Mixed in these proportions all but the poorest material will produce good concrete. Instead of specifying that "the materials shall be of such quality and shall be used in such proportions as will produce a compressive strength of," etc., we now say, in effect, "the materials may be of any quality but must be used in the proportions of approximately 1 : 2 : 4, so that even poor materials will possibly produce concrete sufficiently good for the purpose." They, in principle, are the specifications under which thousands of yards of concrete are laid every year.

Wherein lies the incentive to a contractor or builder to use any better materials than the cheapest, if he is compelled by the specifications to use a certain arbitrary mix regardless of quality or material?

Any specifications for concrete aggregates which are to be used all over this country, must be so drawn that any material which will make concrete of the required quality will be included. The establishment of a standard of quality and insistence that only materials which measure up to that standard may be used, is bound to be ignored in localities where materials of the quality demanded are not to be found, or else the standard of quality would have to be put much lower than at present—so low as to include the poor material.

In operating under such specifications, it is of great importance that specimens of the concrete produced on the job be regularly made and tested. It is also of the greatest importance that a close day-to-day check be maintained on the quality of the materials used, so as to insure a reasonable uniformity, and to know that these materials are at least equal in quality to the materials used in arriving at the proportions required to give the quality of concrete called for in the specifications.

When the materials used on the job are from the same sources as those tested and from which tests the proportions to be used were determined, it is a simple matter to check up their qualities. Sand and stone from the same source do not vary much in quality, except in so far as quality is influenced by size of particles. Having once established by test the suitability of sand and stone for any grade of concrete and having determined the proper proportions in which to use them to attain a certain desired result, it is only necessary thereafter to see that the size, grading and proportions of these materials are reasonably constant to insure uniform quality of concrete. Such a check on size and grading should be had on each and every shipment of material and is easily obtained with a small set of sieves, or in the case of sand, which is by far the more important material, by means of a self-contained sand tester.

The regular and systematic testing of the size of the aggregates gives data which will permit the engineer to tell without further tests, whether the aggregates will produce a better or poorer concrete than that produced by the original or standard sample. This fact is based on the well-established principle that, other things being equal, the aggregate whose granulometric-analysis curve most nearly approaches the line of maximum density will produce the best concrete. This makes it possible to determine with reasonable certainty which of two sands of the same kind and from the same source, but differing only in fineness, will make the better concrete. These principles are carried out in practice by Westinghouse Church Kerr and Co. in the following manner:

Concrete is to be placed in a certain locality. There are to be heavy machinery foundations and thick building foundation

walls and footings below grade, with reinforced superstructure. The engineer in charge secures samples of the available concrete aggregates, both fine and coarse, and sends them to the laboratory for test. The tests show that although the best available sand has a strength in 1 : 3 mortar only 70 per cent of that of standard Ottawa sand, yet mixed in the proportions of 1 : $1\frac{3}{4}$: $3\frac{3}{4}$ with the cement and coarse aggregates to be used, the resulting concrete has a compressive strength of 2600 lb. per sq. in. at 28 days. Other proportions give higher and lower strengths, depending on their richness, but as the design of the structure requires concrete having a compressive strength of 2500 lb. per sq. in. the 1 : $1\frac{3}{4}$: $3\frac{3}{4}$ proportion is used. For the foundations and footings, the designs being based on a compressive strength of 1500 lb. per sq. in. in the concrete, the proportion of 1 : $2\frac{1}{4}$: $4\frac{1}{2}$, which gave in the test a compressive strength of 1550 lb. per sq. in., is chosen. Under the present standard method of specifying sand, this particular sand could not have been used in concrete.

Among the tests made in the laboratory on the sand are several granulometric-analysis charts made with the sand tester. This sand tester has five screens having 6, 10, 20, 35, and 65 meshes per inch. Each screen in succession has openings one-half the width of the openings in the preceding screen. The charts are averaged and a special guide chart is prepared for the use of the inspector on the job. In making up this guide chart a permissible variation of about 2.5 per cent each way from the mean of the tests made on the sample, is allowed. A copy of this chart is sent to the job and a copy kept in the laboratory.

As the sand arrives on the job the inspector, or some one designated by the superintendent, makes tests with the sand tester and compares the resulting chart with the guide chart. If the results show greater variation than is permissible,—particularly if they show the sand to be finer than shown on the guide chart,—then the matter is taken up with the one who supplies the sand. A duplicate chart of all field tests is sent to the engineer.

By this method the quality of the aggregates is recognized and provided for in the selection of proportions for the concrete,

and enough cement is used to produce the desired quality. All the materials entering into the concrete are used in the tests and the results are influenced by each of the ingredients. In this way the uncertainty which is attendent upon separately testing each of the three materials, and predicting therefrom the quality of the concrete resulting from their combination, is eliminated. The time required for testing the combination is no greater than that required for testing any one of the materials.

Experience with this form of specification has shown it to be advantageous in localities where the available aggregates are not up to the standards now specified.

DISCUSSION.

Mr. Ferguson.

MR. L. R. FERGUSON.—This very interesting and valuable paper is of particularly timely interest. It brings out the necessity of giving greater attention to the aggregate in concrete.

During the preparation recently of the proposed revised standard specifications for Portland cement, the effect of very small variations in the chemical and physical properties caused lengthy discussion. Too often cement is used by the contractor without any regard at all to the quality of the aggregates with which it is combined. In what is practically the richest mixture used, the 1:2:4 mixture, the cement forms only one-seventh of the total volume of the separate materials, exclusive of water, which go to make the concrete. That one-seventh is very carefully made and tested, and the other six-sevenths receives little or no attention.

The paper brings out the fact that in some instances it is necessary to import aggregates from a distance, if good concrete is to be secured. Reference might have been made to the possibility of improving local aggregates. Very often this can be done by washing or grading, and that point is very little appreciated by engineers at large.

Frequently our specifications deal with aggregates by simply stating that the fine aggregate shall be coarse, clean and free from loam or other deleterious materials. The engineer then feels that his conscience is clear, and the contractor uses anything he pleases. We certainly ought to insist that more attention be given to the quality of the fine aggregates, but care must be taken that some fine aggregates are not discriminated against unjustly. I have known of cases where, by either a visual or sieve analysis, one would be justified in condemning a fine aggregate according to our present knowledge, and yet, when that fine aggregate was combined with the coarse aggregate to be used it proved perfectly satisfactory.

The paper also brings out the point that an incentive should be given to contractors to use a good quality of fine aggregate and care in proportioning their concrete. Our present building laws

are in many instances unfortunate in that respect. They usually prescribe arbitrarily the mixtures to be used for various classes of work, and the contractor, who may be anxious to produce the very best results, is thus given small option. All contractors are on practically the same basis, owing to the fact that the proportions are specifically fixed, and the allowable strengths of the resulting concrete are also fixed. More credit should be given good workmanship, allowing for instance, higher stresses. **Mr. Ferguson.**

The superintendents on the job, the men actually in charge of the work, need a great deal of education. I ran across an interesting case out West last year. In some localities there they are having a good deal of trouble with their concrete. The engineer was familiar with the conditions, he knew that care should be exercised, and he told me that they were using the utmost precaution to secure proper grading. He said he never used bank-run material, but always insisted that it be sieved and recombined in the proper proportions. And yet, when later we talked to the foreman, the latter said, "What is the use of sieving the thing apart and then shoveling the parts together again? I never do it." That man's education in concrete started at that moment.

MR. R. J. WIG.—I heartily agree with the suggestion of the author. For several years we have been of the opinion that a test cannot be made on a sample of sand alone from which its quality in concrete can be predicted, except in a very general way. **Mr. Wig.**

There are, however, two exceptions I would take to the author's statements:

1. The comparative tensile strength of the commercial sand and standard Ottawa sand in cement mortars does not give a true indication of the probable value of the commercial sand when used in concrete. The sand having the lower tensile strength may be of superior quality when used in concrete.

2. The quality of the concrete is not controlled by fixing the proportions and selecting materials. The quantity of water used and the workmanship employed in fabrication may cause 50 per cent or more variation in strength.

MR. CLOYD M. CHAPMAN.—I am obliged to disagree with Mr. Wig's statement with reference to the value of the strength **Mr. Chapman.**

Mr. Chapman. test of sand. I am firmly convinced that we can judge the quality of a sand to be used in concrete from tests of mortar. My conclusion is based on a study of a very large number of sands from a great many different localities in the United States. The quality of the mortar produced will depend upon the size, the grading, the cleanness of the sand, and on the amount of water and cement used. If the influence of each of those variables is understood, there is no reason why the sand should act differently in mortar mixtures than in concrete mixtures. I am of the opinion that we can judge the quality of a sand from mortar tests.

In further reply to Mr. Wig, I realize that the question of water is a very important one. I have covered it by saying that the aggregate shall be tested according to "the standard methods of testing," which standard methods include the subject of consistency, or the amount of water to be used, and other details of the laboratory methods to be followed in order to duplicate field results. This is all in the paper; it was not missed, but was just covered by the four words, "Standard Methods of Testing." These words are elastic enough to include all such details.

Mr. Long. MR. W. F. LONG (*by letter*).—The formula for the quantities for one cubic yard of concrete to be developed in this discussion applies to any proportions by which materials are to be mixed to make the concrete, and is based upon the determination of the voids in the sand and stone that may be used. First, an expression is found to represent the absolute volumes of cement, sand and stone in a cubic yard of concrete; the sum of these volumes equaling 1 cu. yd.

Thus, in any cubic yard of properly mixed concrete, we can imagine separating the stone aggregate and compacting the same until it is in one solid mass, which represents the absolute volume of the same. We may likewise conceive that the sand and cement are recompacted so as to represent their respective absolute volumes. If there are then no spaces occupied by either water or air, the sum of these three absolute volumes is equal to 27 cu. ft.

Let C = number of cubic feet of cement (measured loose) required for 1 cu. yd. of concrete;

a = the sand factor; and

b = the stone factor.

Mr. Long.

Then aC = number of cubic feet of sand (measured loose) required for 1 cu. yd. of concrete;

bC = number of cubic feet of stone (measured loose) required for 1 cu. yd. of concrete.

Let V_s = number of cubic feet of voids in 1 cu. ft. of sand (measured loose);

V_g = number of cubic feet of voids in 1 cu. ft. of stone (measured loose);

Then $aC\ V_s$ = total number of cubic feet of voids in aC cu. ft. of sand;

$bC\ V_g$ = total number of cubic feet of voids in bC cu. ft. of stone.

Eqs. (1) and (2) may now be written:

$aC - aCV_s$ = absolute volume of sand ..... (1)

$bC - bCV_g$ = absolute volume of stone ..... (2)

Since experiment has shown that 0.9 cu. ft. of cement paste can be obtained from 1 cu. ft. of cement (measured loose), and further assuming that the volume of cement paste represents the absolute volume of the cement, then Eq. (3) follows:

$0.9C$ = absolute volume of cement ..... (3)

The sum of Eqs. (1), (2) and (3) should equal 27 cu. ft. or,

$$(aC - aCV_s) + (bC - bCV_g) + 0.9C = 27.$$

Thus

$$C = \frac{27}{a(1-V_s) + b(1-V_g) + 0.9} \quad \ldots\ldots (4)$$

Example: Required the quantity of materials in 1 cu. yd. of concrete. Mix 1 : 2 : 3½; voids in sand = 30 per cent; voids in gravel = 45 per cent. Here we have $a = 2$; $b = 3.5$; $V_s = 0.3$; $V_g = 0.45$.

Hence

$$C = \frac{27}{a(1-V_s) + b(1-V_g) + 0.9} = \frac{27}{2(1-0.3) + 3.5(1-0.45) + 0.9}$$

$$= \frac{27}{4.225} = 6.390.$$

Mr. Long. That is,

$C = 6.390$ cu. ft., or allowing 3.8 cu. ft. per barrel, we have

$$\frac{6.390}{3.8} = 1.68 \text{ bbl. cement required} = 6.4 \text{ cu. ft.}$$

$$\text{Sand required} = aC = \frac{2 \times 6.390}{27} = 0.47 \text{ cu. yd.} = 12.78 \text{ cu. ft.}$$

$$\text{Stone required} = bC = \frac{3.5 \times 6.390}{27} = 0.83 \text{ cu. yd.} = 22.4 \text{ cu. ft.}$$

For use in practice the writer has made tables which are readily computed and have been found to be of great aid in making estimates. It is to be noted that account is taken of the variation of the voids in the sand, which is often neglected, giving rise to error in the quantities required and actually used.

Mr. Chapman. MR. CHAPMAN (*Author's closure by letter*).—Mr. Long's formula for determining the quantities of materials required for one cubic yard of concrete is simple and direct in its form, but as at present demonstrated it lacks one vital feature, that is, a method of determining the percentage of voids in the sand and the stone. The formula is based on the assumption that the percentage of voids is a known quantity or can be ascertained, but no method of determining it is included in Mr. Long's discussion, nor is there any method as yet in general use which can be called standard. With a given sand the percentage of voids depends upon the amount to which the sand is compacted when the determination of voids is made. For example, with the same sand the writer has obtained results varying from 29 to 57 per cent, depending upon the degree to which the sand is settled down in the container in which voids are determined.

But this is not the greatest difficulty. The quantity of materials required to make a cubic yard of concrete depends not upon the percentage of voids in the sand and the stone as determined separately, but upon the voids between the stone as it lies in the concrete and the voids between the sand particles as it lies in the concrete. In this connection the term "voids" is used to express the space between the particles which in the case of the stone is occupied by sand, cement, water and air, and in the case of sand, by cement, water and air. A determination of the space

between the pieces of stone in the container in which there is nothing but stone will give a very different result than a determination of the space between the pieces of stone in a concrete. When the voids are determined using stone alone, each piece of stone rests against its neighbor, whereas in the concrete it is separated by the mortar, so that the voids are necessarily greater in the case of concrete than in the case of the single aggregate. How much greater, has not yet been reliably determined. But it is this percentage of voids which is required in the formula of Mr. Long rather than the voids as determined by test of the separate materials, and until a method is put forward and adopted for determining the space between the stone pieces and the space between the sand particles as these materials finally lie in place in the finished concrete, the formula proposed by Mr. Long will be of interest but of relatively little practical use. Mr. Chapman.

A METHOD OF MAKING WEAR TESTS OF CONCRETE.

By D. A. Abrams.

SUMMARY.

The extensive use of concrete in the wearing surface of roads and pavements gives interest to wear tests of concrete.

A brief review is given of tests which have been used for determining the wearing resistance of concrete aggregates and concrete.

A method is described for conducting wear tests on concrete blocks 8 in. square and 5 in. thick by means of the Talbot-Jones rattler.

This method is believed to offer the following advantages:

1. The concrete is subjected to a treatment which approximates that of service.

2. The test piece is of usual form and of sufficient size that representative concrete can be obtained.

3. The test pieces are convenient to make, store and handle, and require a relatively small amount of concrete.

4. The cost of tests is not excessive.

5. The machine used is found in numerous testing laboratories.

6. The wearing action takes place on the top or finished surface of the concrete. This makes it possible to study the effect of various surface treatments or finishes.

7. Several tests may be made at the same time, thus enabling more representative results to be obtained.

8. Tests may be made on sections of concrete cut from roads which have been in service.

9. Other paving materials such as brick, granite blocks, etc., may be tested in the same way as the concrete.

A METHOD OF MAKING WEAR TESTS OF CONCRETE.

By D. A. Abrams.

The extensive use of concrete as the wearing surface of roads and pavements has given renewed interest to experimental studies of the wearing resistance of concrete.

If weather resistance and structural stresses are properly provided for, the life of a road will depend on the wearing resistance of the material of which it is built. Satisfactory wearing resistance is one of the most important considerations of any materials to be used in road surfaces.

Wear, in the sense in which the term is here used, results from combinations of stresses such as abrasion, impact, bearing and crushing. Wearing resistance is a function of the hardness, toughness, brittleness, etc., of a material. In the case of homogeneous materials, such as natural rocks, it may be satisfactory to study the individual properties mentioned above; however, with a material of the nature of concrete, it is preferable to study the wearing resistance directly, without reference to the value of the more elemental properties.

Numerous testing machines have been designed for use in determining the abrasive resistance of natural rocks, cement mortars and brick. The following are typical of the methods which have been used:

Dorry Hardness Testing Machine.—The Dorry hardness testing machine has been used for natural rocks. The test pieces consist of 1-in. cylinders cut out by means of a core drill. The cylinders are tested in pairs by being held in a vertical position with a constant pressure against a revolving metal disk. The abrasive agents are crushed quartz and water. The hardness of the material under test is measured by the depth of wear. This test was devised by the French School of Bridges and Roads, and used by United States Office of Public Roads and Rural Engineering for tests of rocks for macadam road

construction.[1] This test is carried out with considerable difficulty and is of doubtful value as an indication of the useful properties of a rock; it is not adapted to testing concrete aggregates in the form in which such materials are delivered for use, the specimen being too small for use in mortar and concrete tests.

Machines with a cylindrical drum against which the test piece is held have been used in carrying out tests of the same nature as those made in the Dorry machine.[2]

Sand Blast.—The sand blast has been used for testing the wearing qualities of natural rocks, bricks, etc.

On account of the absence of the element of impact, the above-mentioned methods are not satisfactory for tests of concrete.

Deval Abrasion Testing Machine.—The Deval abrasion testing machine for crushed rocks consists of a closed metal cylinder mounted in an inclined position on a horizontal axis which is caused to revolve at a fixed rate. The test sample consists of about 50 pieces of crushed rock weighing 5000 g. The abrasive action is derived from the falling and rubbing of the particles on each other and on the walls of the cylinder, 10,000 revolutions at about 30 r.p.m. constituting a test. The percentage of wear is computed from the weight of material which is reduced to a size smaller than a $\frac{1}{16}$-in. sieve. This test was devised by the French School of Public Bridges and Roads, and has been adopted by the American Society for Testing Materials as a standard test for abrasion of road materials.[3] The indications of this test have been widely used as a criterion of the value of rocks for macadam road building. The test is of doubtful value in indicating the suitability of an aggregate for concrete road construction. The size of pieces required by the present standard test is not readily obtainable from the aggregates commonly used in concrete road construction. It is not applicable to tests of gravel.

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[1] For a description of this machine, see *Bulletins Nos. 44, 49 and 347*, United States Office of Public Roads and Rural Engineering.

[2] "Note on Appliances for Testing Paving Setts," by Labordère and Anstett, *Proceedings* Sixth Congress, Int. Assn. Test. Mats., 1912, Second Section, paper XIX2.

[3] "Standard Test for Abrasion of Road Materials," 1915 Year-Book, p. 440.

A modified form of the standard Deval test has been used for testing gravel by the Ohio State Highway Department. In this test 2500 g. of gravel between the $\frac{1}{2}$ and 1-in. screens, and a similar weight of material between the 1 and 2-in. screens, are used with an abrasive charge which consists of six cast-iron spheres $1\frac{7}{8}$ in. in diameter. Otherwise the test is conducted in the same way as the standard test.[1]

Several attempts have been made to use the Deval machine for studying the wearing resistance of mortars and concretes. The test pieces used were small cubes or spheres of mortar or concrete, which were placed in the test chamber in sets of three or four, with or without a charge of metal shot. On account of the small test pieces which must be used, these tests have not proved entirely satisfactory.

Development of Rattler Test.—The development of the rattler test for paving brick is of interest in this connection. The present standard rattler is an adaptation of the ordinary foundry rattler. The Talbot-Jones rattler was designed for testing paving brick, but was not officially adopted as a standard method.[2]

A "pavement determinator" was on exhibition in Detroit and at the Cement Show in Chicago in 1913, for use in testing sections of finished pavements. This was a motor-driven mechanism which subjected the pavement to impact stresses closely approximating the action of horses' hoofs.[3] The cost of this method would make it prohibitive for either research or routine testing.

As early as 1913 Professor Charles F. Shoop, of the University of Minnesota, made use of the Talbot-Jones rattler for studying the wearing resistance of concrete.[4]

In the tests carried out by Professor Shoop, the test piece was a ring 28 in. inside diameter, 4 in. thick and $8\frac{1}{2}$ in. wide. These rings were cast in a horizontal position with the axis vertical, and were reinforced with two circular steel bars.

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[1] General Specifications for Materials, No. 3, 1915, Ohio State Highway Department.

[2] For an account of the use of the Talbot-Jones rattler, see *Proceedings*, National Brick Manufacturers' Association, 1899, 1900 and 1901; also, "The Testing of Road Materials," L. W. Page, *Bulletin No. 79*, U. S. Bureau of Chemistry (1903).

[3] *Concrete Cement Age*, December, 1912.

[4] "An Investigation of the Concrete Road-Making Properties of Minnesota Stone and Gravel," Charles F. Shoop, Studies in Engineering, No. 2, University of Minnesota (1915).

EXPERIMENTS WITH TALBOT-JONES RATTLER.

Method of Testing.—The writer is now making studies of the wearing resistance of concrete at the Structural Materials Research Laboratory, Lewis Institute, Chicago, in which the Talbot-Jones rattler is being used. The machine is shown in Fig. 1. The test pieces consist of blocks 8 in. square and 5 in. in thickness. The blocks are arranged around the perimeter

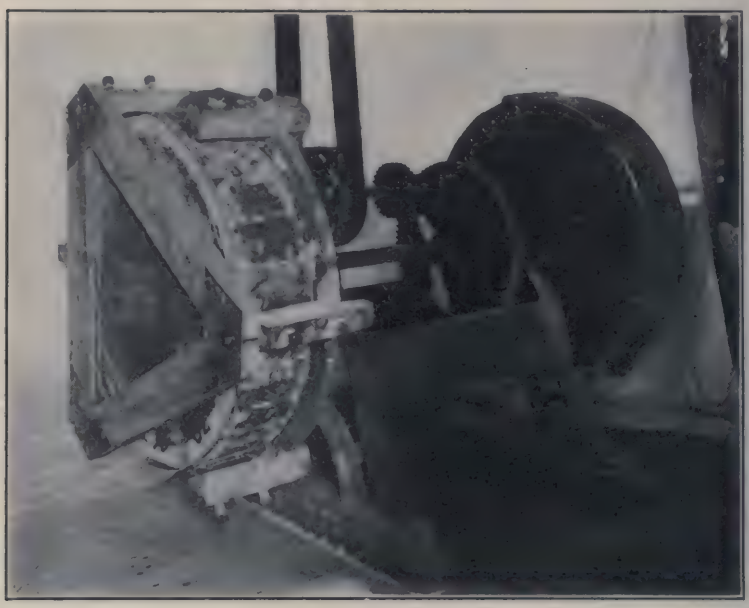


FIG. 1.—Talbot-Jones Rattler Used for Wear Tests of Concrete.

of the drum of the rattler as shown in the accompanying illustrations. Ten blocks constitute a test set. The concrete test pieces are separated by wedge-shaped wood blocks. The ten-side polygon formed by the test blocks presents a nearly continuous inner surface, as shown in Fig. 2. The outside diameter of the polygon thus formed is 36 in., and the inside diameter 26 in. During the test, the front of the chamber is closed by means of a heavy wire screen.

The abrasive charge consists of 200 lb. of cast-iron spheres,

about 133 spheres $1\frac{7}{8}$ in. and 10 spheres $3\frac{3}{4}$ in. in diameter. These spheres conform to the specifications for spheres for use in the standard rattler test of paving brick.[1] The test consists of exposing the inner faces of the concrete blocks to the wearing action of the charge of cast-iron spheres for 3600 revolutions, at the rate of about 30 r.p.m. Best results have been obtained by reversing the direction of the machine two or three times during the test. Each block is weighed immediately before and

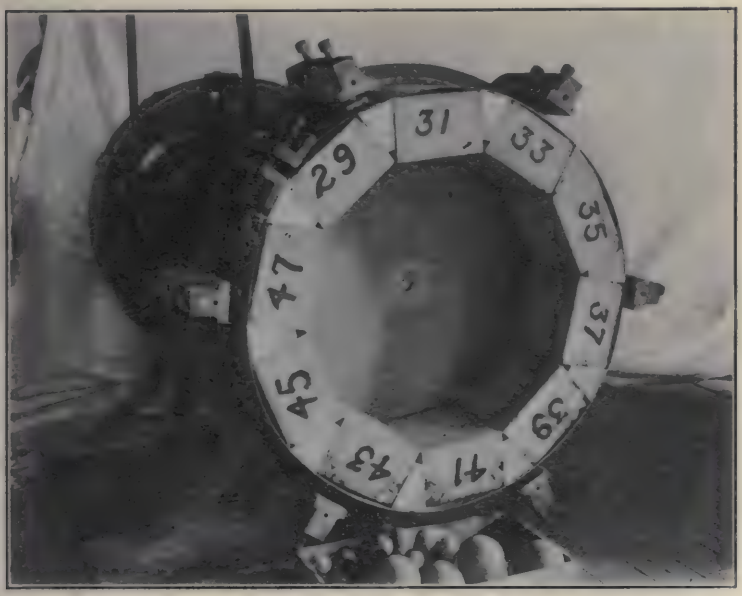


FIG. 2.—View of Blocks before Test. Blocks 8 in. Square, 5 in. Thick.

immediately after the test. The loss in weight is considered to be the wear. This loss may be reduced to a percentage of the original weight, or it may be expressed as depth of wear in inches. In comparing concretes of widely different characteristics, or in comparing concrete with other materials, the latter method is preferable.

Results of Tests.—About 200 concrete blocks have been tested in this way. The tests thus far made have been carried out

[1] "Standard Specifications for Paving Brick," 1915 Year-Book, p. 396.

primarily for the purpose of studying the action of the machine. The weight of shot to be used, the proportion of small and large spheres, the rate and number of revolutions, the effect of the position of the block in the ring with reference to other blocks of the same or widely different properties, have been studied. A few variations in the mix, aggregates, condition of storage of the concrete, etc., have been made, but the tests are not considered to be of sufficient scope to justify presenting the results

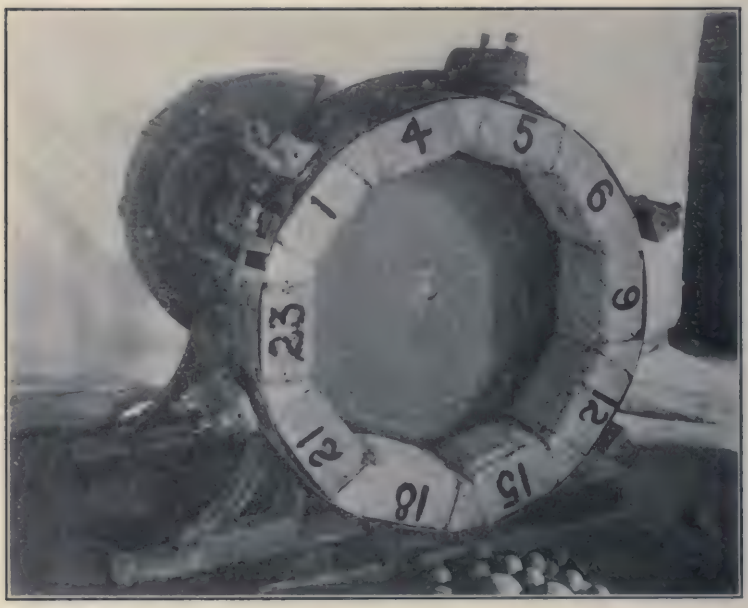


FIG. 3.—View of Blocks after Completion of Test.

at this time. The loss in weight due to the test has varied from 8 to 25 per cent. Figs. 3 and 4 show the appearance of representative blocks after the test. A few paving brick have been tested in the same ring as the concrete blocks, with satisfactory results.

In making the concrete test blocks, it is our custom to proportion and mix each block separately. Only in this way can consistent results be secured. Metal forms made from 5-in. steel channels are used. These forms make it possible to mold

the blocks of uniform size and true to shape. Blocks which have been stored in water or in damp sand are allowed to dry out a few hours prior to test, in order that the weights may not be appreciably affected by evaporation during the test.

Advantages of Rattler Test.—This method of making wear tests of concrete is believed to have the following advantages:

FIG. 4.—Concrete Blocks after Test. Blocks 8 in. Square, 5 in. Thick. Age about 5 Months.

Nos. 6, 7, 8	1:1½:2½ gravel concrete.
Nos. 12, 13, 14	1:2:3 gravel concrete.
Nos. 15, 16, 17	1:2:3 limestone concrete.

1. The concrete is subjected to a treatment which approximates that of service.

2. The test piece is of usual form and of sufficient size that representative concrete can be obtained.

3. The test pieces are convenient to make, store and handle, and require a relatively small amount of concrete.

4. The cost of the tests is not excessive.

5. The machine used is found in numerous testing laboratories.

6. The wearing action takes place on the top or finished surface of the concrete. This makes it possible to study the effect of various surface treatments or finishes.

7. Several tests may be made at the same time, thus enabling more representative results to be obtained.

8. Tests may be made on sections of concrete cut from roads which have been in service.

9. Other paving materials such as brick, granite blocks, etc., may be tested in the same way as the concrete.

The tests indicate that this method may prove of considerable value in studying the relative merits of different aggregates, mixes, consistencies, time of mixing, surface treatment, etc., on the wearing resistance of concrete. It can readily be used as a control test on blocks made at frequent intervals as the work progresses.

The writer is indebted to Prof. Ira O. Baker and Prof. A. N. Talbot, of the University of Illinois, for their courtesy in lending the Talbot-Jones rattler which was used in these experiments.

DISCUSSION.

MR. G. P. HEMSTREET (*presented in written form and read by the author*).—For upwards of ten years, the Hastings Pavement Co. has been using a modified form of Talbot-Jones rattler for testing asphalt paving blocks. Numerous tests have also been made on vitrified brick and some tests have been made on concrete blocks. Some years ago, a similar apparatus was installed at New York University. This same apparatus has been used by at least four other manufacturers of asphalt blocks. Mr. Hemstreet

The original Talbot-Jones rattler was modified as follows:

1. By improving the method for clamping the block in place;

2. Installing the apparatus in an insulated chamber and providing means for artificial cooling, or heating or wetting the rattler and its contents during the test, so that climatic conditions can be reproduced;

3. Installing an especially accurate tachometer to indicate accurately the speed of revolutions, and means to maintain the desired speed;

4. Substituting a new charge, for each test, of 50 lb. of $2\frac{1}{2}$-in. cast-iron cubes, for cubes or spheres of various sizes;

5. Revolving the rattler at exactly 37 r.p.m. for 10,000 revolutions.

Fig. 1 shows the improved form of rattler at the conclusion of a test. The disk covering the front of the cylinder has been removed, exposing the worn material of the pavement and the worn cubes. Note the clamps for holding the blocks or bricks in place; ammonia coils surrounding the drum; the insulated chamber surrounding the entire apparatus; the refrigerating machinery in the rear.

The Hastings Pavement Co. uses two machines and routine tests of their product are made daily, and, during the busy paving season, the apparatus is used night and day. A cold and hot test is made for each twelve hours run.

During the winter months, many tests are made on experi-

Mr. Hemstreet.

mental mixtures which have been prepared during the fall before the factory has closed.

The blocks are weighed before and after the test and the results are expressed in pounds of loss per square yard of pavement. After the test, the blocks are carefully examined and their appearance is described as No. 1, 2, 3, etc. A paving mixture may show a comparatively small loss in pounds per square yard but its appearance, after the test, would be so ragged that it would be unsatisfactory and unsightly on the street.

Fig. 2 shows various kinds of asphalt block after testing

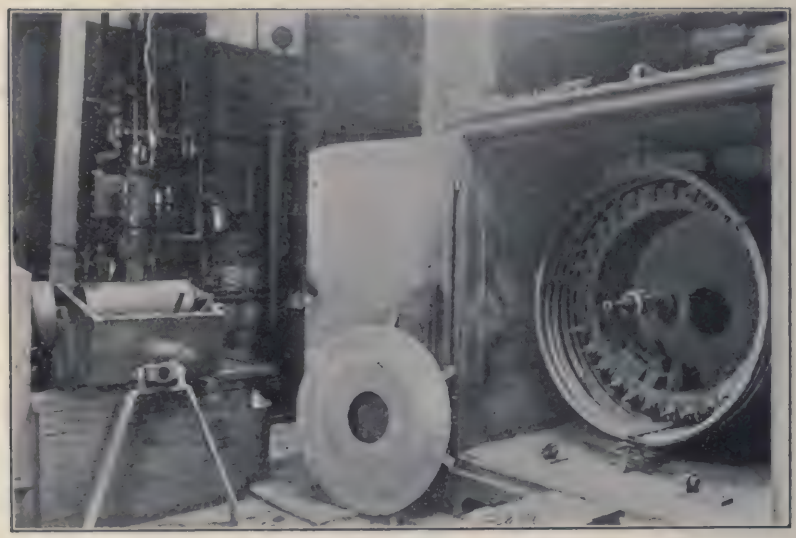


FIG. 1.—Modified Talbot-Jones Rattler.

in the rattler. The best blocks are at the right of the illustration. These blocks have worn evenly, the surface has not pitted and the edges are intact. The others show varying degrees of wear, more or less pitting on the surface, and broken edges. The blocks on extreme left show excessive wear and roughness.

Many thousands of tests have been made, and from the experience thus gained, valuable conclusions have been drawn as outlined below:

1. The improved apparatus approximates actual traffic and climatic conditions.

2. Considerable skill and experience and careful attention to detail are necessary in order to operate the rattler so as to get concordant results. Mr. Hemstreet.

3. In the hand of a skillful operator, the apparatus yields very valuable and uniform results when testing paving material that wears away by impact and abrasion. Similar material tested by different operators on different apparatus will give practically identical results.

4. Material which shows up poorly in the rattler will invariably give poor results on the street, and, as a general rule, those materials which give good results in the rattler give good results on the street, although there are occasional exceptions to this rule.

5. In judging the value of paving material, it is necessary to consider not only the numerical results obtained but the

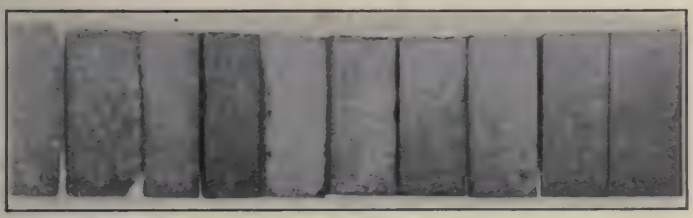


FIG. 2.—Various Asphalt Blocks after Testing.

appearance of the material after the test, together with a careful consideration of the climatic and traffic conditions under which the pavement is laid.

6. The improved apparatus has given very valuable results and has been one of the means which has enabled those manufacturers of asphalt paving blocks who use it to very greatly improve the quality of their product.

7. The test, as described, approximates ten years wear on highways carrying an average of 5000 vehicles per day. For example, the wear on asphalt blocks on such busy thoroughfares as Pelham Parkway, New York City, and Albany Post Road and the shore road from New York to Boston, has been $\frac{1}{8}$ in. in five years, while the wear in the rattler described is approximately $\frac{1}{4}$ in. over the surface. These roads average 5000 vehicles per day, sometimes 1500 per hour.

The improved apparatus described is much more severe in

Mr. Hemstreet. its action on the material tested than that described by Mr. Abrams in his paper.

When testing with 50 lb. of cast-iron cubes at 37 r.p.m. for 10,000 revolutions, an average loss on asphalt block pavement is about 25 lb. per sq. yd. or approximately $7\frac{1}{2}$ per cent. When testing the same blocks with spheres at 30 r.p.m. for 3600 revolutions, as described in Mr. Abrams' paper, the loss is only $\frac{3}{4}$ lb. per sq. yd. or about 0.23 per cent, so that the wear with the cubes is approximately thirty times as great as with the spheres. Cubes more nearly reproduce actual traffic conditions, as the corners of the cubes dig into the pavement like sharp caulks on horses, while the whole mass of cubes, simulating traffic, grinds away the surface.

For the sake of uniformity, it would be well if Mr. Abrams could use 50 lb. of cubes for 10,000 revolutions, and thus be able to directly compare his results with those of several thousands of tests made by various manufacturers on different paving materials.

However, as his tests show that the losses which he obtained on concrete run from 8 to 25 per cent, it would doubtless be impracticable to use cubes for the full 10,000 revolutions, as this would subject his material to a test approximately thirty times as severe, which would probably destroy the material completely.

Mr. Mattimore. MR. H. S. MATTIMORE.—I think the test presented by Mr. Abrams is a distinctly timely one. I want to emphasize especially the points in regard to the wet test to which Mr. Hemstreet has referred. Some comparative tests have been made on both stone and mortar under wet and dry conditions, and it would seem valuable data might be obtained by following this up with concrete.

As a suggestion, I should like to inquire from Mr. Abrams if he has considered some method by which the test he describes could be run wet.

The cold and hot test mentioned by Mr. Hemstreet is a valuable one for asphalt products, but I fail to see that it has any relation to concrete materials.

Mr. Abrams. MR. D. A. ABRAMS.—I was very much interested in Mr. Hemstreet's account of his experiments. I was not aware of the special work he has been doing along this line, although I have

been familiar for several years with the use of the Talbot-Jones rattler in tests of paving bricks. Mr. Abrams.

I mentioned in the paper that this is merely a preliminary discussion of the method. We have not arrived at any definite conclusions as to the relative value of different concrete materials or anything of that kind, although we are planning to continue our studies along these lines.

Mr. Hemstreet has pointed out that I have not followed standard practice in some respects. I may say that the use of cast-iron spheres was adopted in preference to cubes or other forms of blocks, for the reason that that is the form of shot specified in the Standard Specifications for Paving Brick of this Society, and it seems to be desirable to stick to one standard as much as possible. I do not see that it would be practicable for us to use the cube, for the reason that the corners and edges soon become rounded and then an entirely different action results. The laboratories making these tests cannot afford to throw away a charge of shot every few tests. The spheres do not materially change their shape and will last a long time. Some people may desire to discard the shot after a few tests, as Mr. Hemstreet does, but for the majority of us it would not be practicable. Furthermore, it does not appear to me that the conditions of traffic are reproduced by using a sharp-edged shot, particularly on a dense, hard material such as concrete.

It seems to me that the amount of wear should not be expressed in terms of pounds per square yard of pavement, since that would depend altogether upon the specific gravity and nature of the material. A better method where materials of widely different character are to be compared, is to express the wear in inches of depth from the original surface. This is entirely independent of the material and is the thing you are interested in. If you have a pavement of 4, 5 or 6 in. in thickness, you want to know how much it may wear and still have a pavement left.

(*Author's closure by letter*).—After reading Mr. Hemstreet's complete discussion, it seems that a few additional observations are desirable.

Mr. Hemstreet states that "the improved apparatus described is much more severe in its action on the material tested than that described by Mr. Abrams in his paper." This tatement is not borne out by any evidence presented by Mr.

Mr. Abrams. Hemstreet. It seems to me that he is undoubtedly in error in this respect and, consequently, that the deductions based upon such an assumption are erroneous. In Mr. Hemstreet's comparison of the effect of cast-iron cubes at 37 revolutions per minute, and of the spheres at 30 revolutions per minute, he apparently neglects to take into account the 200-lb. charge of shot used in this laboratory as against the 50 in his tests. The size of the blocks used by Mr. Hemstreet is not given; this has an important bearing on the amount of wear.

Wear tests made on concrete rings at the University of Minnesota, in which cast-iron cubes and rectangular blocks were used, indicate that the loss due to shot of this kind is probably not materially different from that found in the tests made by the writer in which cast-iron balls were used.

I do not agree that cast-iron cubes with sharp corners reproduce actual traffic conditions any more than spheres do. How many horses carry shoes with caulks similar to the corner of a cube? How many of the 5000 vehicles mentioned above were accompanied by caulks of any kind?

I do not believe the cubes would be appreciably more severe on concrete than the spheres. On the other hand, cubes would probably be less severe on asphalt blocks, since the sharp corners and edges would penetrate the yielding surface and largely reduce the effect of impact.

Mr. Hemstreet mentions the desirability of the writer being able to compare his experimental work with that found by the companies which use his method. It is not clear how this can be done, so long as Mr. Hemstreet does not give any information concerning the results of his tests.

I feel sure that some of the committees of this Society would have been glad at any time during the past five years to have received information which Mr. Hemstreet has on the wearing resistance of road materials.

It should be stated that the concrete which gave a loss of 25 per cent as mentioned in the paper, was purposely dried out prematurely, in order to secure some notion of the effect, and to determine the largest amount of wear that we should provide for. If Mr. Hemstreet produces concrete which shows a wear thirty times as great as asphalt blocks, it seems to indicate at unfortunate lack of familiarity with the methods of making concrete.

THE STRENGTH OF CLAMPED SPLICES IN CONCRETE REINFORCEMENT BARS.

By E. L. Lasier.

SUMMARY.

U-Bolt clamped splices of both 17 and 21-in. lengths of splice were tested to determine the load at first slip, and the maximum load the splices would resist. Three different classes of splices were thus tested:

1. Lap splices not embedded in concrete;
2. Butt splices not embedded in concrete; and
3. Lap splices embedded in concrete.

The reinforcement steel in all cases consisted of 1-in. square cold-twisted bars.

The loads necessary to produce first slip had a range of from 7000 to 50,000 lb. The maximum loads which the splices withstood varied from 23,000 to 69,000 lb. The ratios of load at first slip to yield point of bar for clamped splices not embedded in concrete varied from 12 to 21 per cent, and for splices embedded in concrete from 53 to 83 per cent. Ratios of maximum load to tensile strength of bar ranged from 31 to 61 per cent for unembedded splices, and from 79 to 95 per cent for embedded splices.

It was found:

1. That lap splices are stronger than butt splices;
2. That splices in which the two overlapping bars are of opposite twist apparently are stronger than splices in which the bars are of like twist;
3. That the lengths of lap, as to the two lengths tested, apparently did not affect the strength of the splice;
4. That embedding the splices in concrete increases their strength materially; and
5. That clamped lap splices embedded in large masses of concrete undoubtedly can safely withstand a unit load equal to the unit stress in the steel reinforcement.

THE STRENGTH OF CLAMPED SPLICES IN CONCRETE REINFORCEMENT BARS.

By E. L. Lasier.

Concrete reinforcement bars are received in lengths varying up to about 60 ft. The length most commonly selected, however, is in the neighborhood of 30 ft.; hence, it is frequently necessary in practice to lap one bar over another. It is also desirable upon more or less frequent occasions to secure a splice capable of withstanding a greater tensile stress than that which a plain lap splice of equal length could resist. In such cases U-bolt clamps, such as are ordinarily used in fastening wire cables together, have been successfully employed.

Three years ago such conditions confronted the designers of the wheel chambers of the State Line Power Plant of the Hydro-Electric Co. of West Virginia, on Cheat River near Cheat Haven, Pa. The wheel chambers were of the horizontal scroll type, designed to supply water to vertical, single runner, 12,000-h.p. turbines, operating under an average effective head of about 81 ft., and were unusually large.

The wheel chambers were to be constructed of reinforced concrete, and it was decided to use U-bolt clamps where splices of reinforcement bars were necessary. The meagerness of published data on the subject prompted the investigation herein reported by the writer, who at the time was Engineer of Tests at the State Line development. This scantiness of available data, combined with the feeling that such information might be of value to engineers, also suggested the preparation of this paper. Its presentation has the approval of Mr. F. W. Scheidenhelm, Consulting Engineer, and Chief Engineer of the Hydro-Electric Co. of West Virginia. Mr. George F. Rowell was Engineer-in-charge at the State Line development.

Scope of Investigation.—The tests were divided into three groups. In the first group, two reinforcement bars forming a "lap splice," with two U-bolt clamps, were tested. Secondly, tests were made of two bars placed with the end of one butted

against the end of the other, each clamped by the two U-bolts to a splice bar overlapping each of the main bars, thus forming what will be referred to as a "butt splice." These butt-spliced specimens were prepared and tested in order to avoid, at least partly, the eccentricity of loading resulting in testing clamped lap splices. It was thought that by so doing higher values of load at first slip, and larger maximum loads which the splices would resist, would be obtained; the opposite, however, as will be seen later (Tables III and IV), was the case. In these first two groups, the bars were not embedded in concrete. Thirdly, two bars, forming a lap splice, with two U-bolt clamps, were embedded in a block of concrete and tested.

TABLE I.—MECHANICAL TESTS UPON REINFORCEMENT BARS.

Specimen No.	Description of Specimen.	Sectional Area, sq. in.	Yield Point (Drop of Beam), lb. per sq. in.	Tensile Strength, lb. per sq. in.	After Fracture.		Appearance of Fracture.
					Elongation in 8 in., per cent.	Reduction of Area, per cent.	
1	Original form.....	1.0	63 670	71 370	....	....	Silky.
2	Original form.....	1.0	57 050	74 000	....	....	Silky.
Aver..	.....	.....	60 160	72 685			
3	Turned-down section.	0.738	55 300	78 100	11.5	54.8	Cup-shaped; silky.
4	Turned-down section.	0.710	57 000	78 830	10.9	53.4	Cup-shaped; silky.

NOTE.—In the cold-bend tests, specimens Nos. 5 and 6 were bent 180 deg. about a pin 3 in. in diameter, without signs of flaws.

Materials.—Complete data regarding the properties of all of the materials involved will be given, to permit an intelligent study of the tests.

The reinforcement steel consisted entirely of 1-in. square, cold-twisted bars. The specifications of the Society for steel reinforcement bars, adopted June 1, 1912, as well as the present ones[1] for billet-steel reinforcement bars, cold-twisted, were all fulfilled. Results of mechanical tests upon six specimens of the steel bars are given in Table I.

The average values of yield point and tensile strength as recorded in Table I will be used as a basis for obtaining the

[1] "Standard Specifications for Billet-Steel Concrete Reinforcement Bars," 1915 Year-Book, p. 98.

efficiency of the U-bolt clamped splices, rather than the values given for specimens Nos. 3 and 4. It is of interest to note in passing that the latter values, with a single exception, are larger than the values for specimens Nos. 1 and 2, in about the same proportion as that found by Messrs. E. P. Withrow and

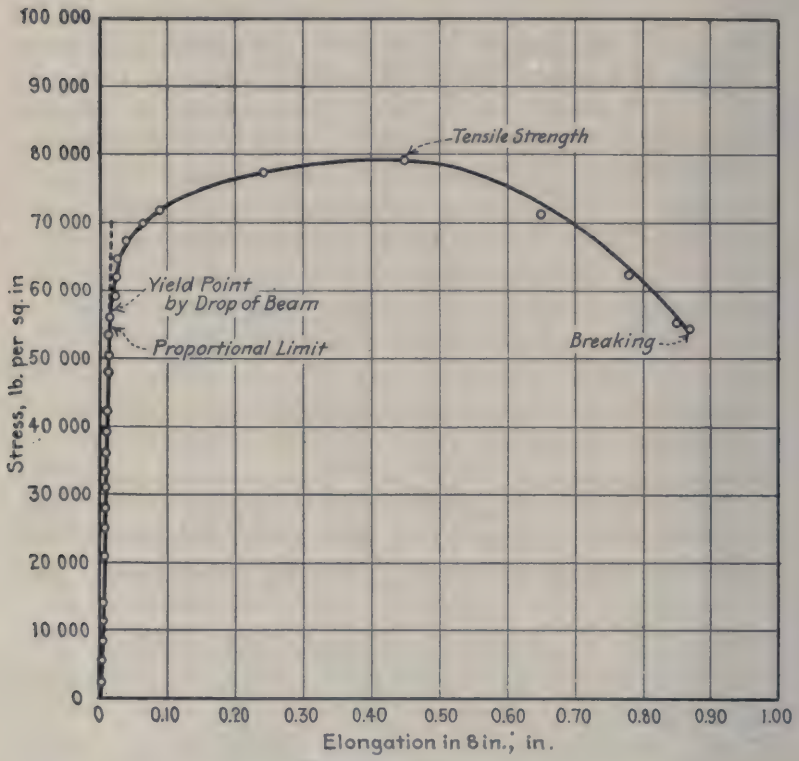


FIG. 1.—Stress-Deformation Curve for Specimen No. 4.

L. C. Niedner in their work on the effect of reducing the area for tension tests of reinforcement bars.[1]

[1] "The Use of Turned Sections in Tension Tests of Reinforcing Bars," *Proceedings*, Am. Soc. Test. Mats., Vol. XIV, Part II, p. 90 (1914). "It is found that there is an increase in the yield point and ultimate strength which may amount to 10 per cent. This is true of constant-section deformed bars. . . . . The indications are that the increase is caused by the chemical and structural non-uniformity of the bars. Since the removal of the exterior steel tends to give a higher reported value for the bars than they would develop in service, it is not good practice to allow the machining of test specimens of reinforcing bars."

The stress-deformation curve of specimen No. 4 is shown in Fig. 1.

The U-bolt clamps used were all steel clamps, $\frac{5}{8}$ in. in diameter, with the following dimensions: clear opening, $1\frac{3}{8}$ by $2\frac{1}{2}$ in., size of steel cross-piece, $4\frac{1}{4}$ by $1\frac{1}{2}$ by $\frac{7}{16}$ in., length of threads, $1\frac{3}{4}$ in. One-inch square nuts were used. Each clamp complete with nuts weighed $1\frac{3}{4}$ lb. The general appearance of the clamps may be seen in Fig. 6.

TABLE II.—ANALYSIS AND TESTS OF CEMENT EMPLOYED.

ANALYSIS.	
SiO_2, per cent	22.76
R_2O_3, per cent	9.78
CaO, per cent	61.36
MgO, per cent	1.80
SO_3, per cent	1.58
Loss on ignition, per cent	1.76

PHYSICAL TESTS.		Mortar (1:3, Standard
TENSILE STRENGTH, LB. PER SQ. IN.	Neat.	Ottawa Sand).
1 day	306	...
7 days	637	239
28 days	762	386

FINENESS.	
Retained on No. 100 sieve	7.6 per cent
Retained on No. 200 sieve	24.6 per cent
SETTING TIME.	
Initial set	4 hr. 30 min.
Final set	6 hr. 50 min.
SOUNDNESS.	
Cold-water test.....Passed.	Boiling test.....Passed.
SPECIFIC GRAVITY.	
Specific gravity	3.14

The concrete used was a 1:2:4 mixture, and was taken from the concrete being poured around the draft tubes at the same time the test specimens were being prepared. It was not a laboratory-mixed batch, but was mixed and delivered under conditions similar to that encountered on the work.

The cement was Alpha Portland, meeting the requirements of the present specifications of the Society.[1] A typical analysis is given in Table II.

[1] "Standard Specifications for Portland Cement," 1915 Year-Book, p. 350.

The sand was very nearly a 1:1 mixture of Monongahela River and Cheat River crushed sand. It had a specific gravity of 2.54, a silt content of about 4.0 per cent, a void space of about 40 per cent, and weighed approximately 95 lb. per cu. ft. The granulometric analysis curve is shown in Fig. 2.

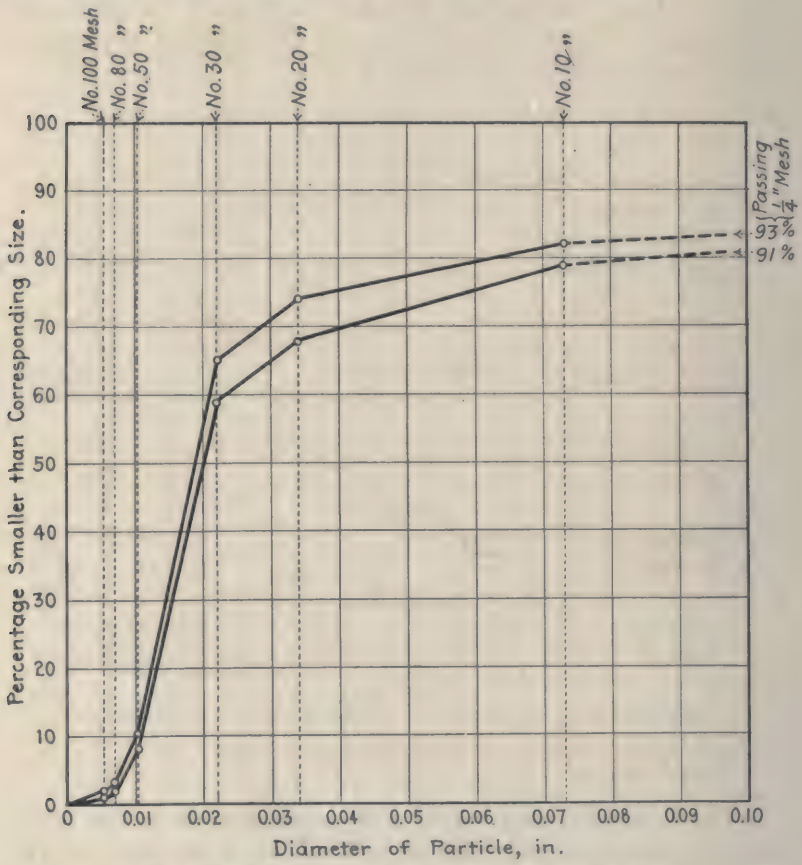


FIG. 2.—Granulometric Analysis Curve for the Sand.

The coarse aggregate used was Cheat River crushed gravel, having a specific gravity of 2.62, a void space of about 45 per cent, and weighed approximately 91 lb. per cu. ft. The mechanical sieving analysis curve is shown in Fig. 3. The general character and texture of the gravel may be seen from

Fig. 4. Some of the pieces of aggregate there shown were taken from broken concrete test cylinders, and show the adhesion of mortar; other pieces were taken directly from the crusher.

Description of Test Specimens.—The three groups of tests have already been described. Specimens in each group were

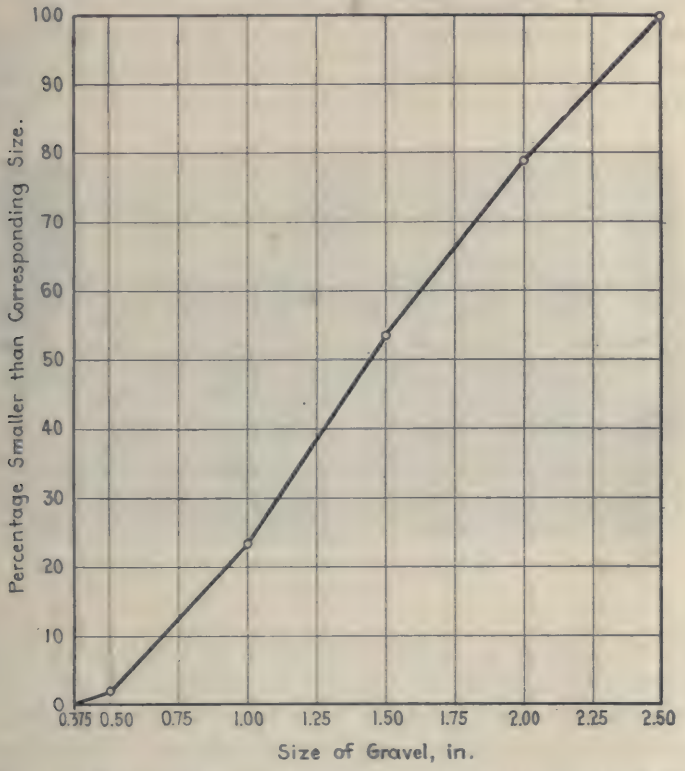


FIG. 3.—Mechanical Sieving Analysis Curve for the Coarse Aggregate.

tested with lengths of lap of 21 and 17 in.; the total lengths of lap in the cases of the butt splices were correspondingly 42 and 34 in. These lengths were selected by the engineers in charge as being those best adapted from practical considerations for the work in hand. The two different lengths of lap were tested in view of the possibility that, because of the different number of spirals which would be brought into con-

tact with varying lengths of lap, an effect might be produced in the holding power of the splice. Also, it was thought desirable to test laps of these two particular lengths, since if the longer lap were found stronger than the shorter, neither the extra length of steel reinforcement bar required for the longer lap was excessively uneconomical, nor was the longer splice inconvenient for use for practical reasons.

Fig. 5 shows the character of the third group of specimens—those embedded in concrete. (The general appearance of the first and second groups may be seen from Figs. 8 and 7, respect-

FIG. 4.—Pieces of Aggregate taken from Broken Concrete Test Cylinders.

ively, although it is to be understood that specimens in the first group of tests had no concrete embedding the splice.)

Specimens Nos. 7 to 10, inclusive, consisted of clamped lap splices, not embedded in concrete, with two U-bolt clamps, each one placed 4 in. from the ends of the splice. Two of these specimens had a 21-in. lap; two had only a 17-in. lap. For each length of lap, there was one specimen consisting of the reinforcement bars having like-direction twist, namely, either both clockwise or both counterclockwise; and also one specimen with bars of opposite twist, namely, one clockwise and the other counterclockwise.

Specimens Nos. 11 to 14, inclusive, consisted of clamped butt splices, not embedded in concrete. Two of these specimens

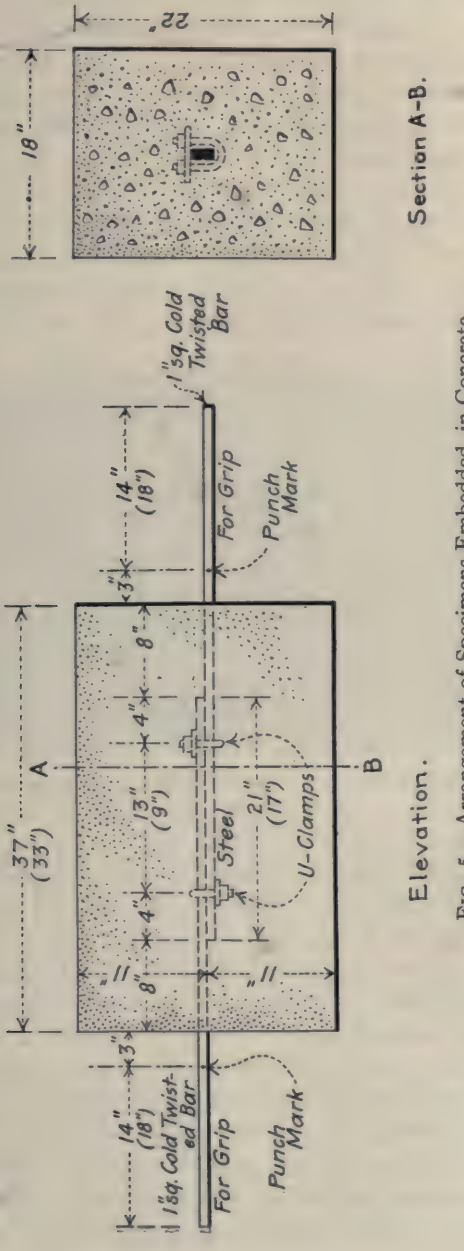


FIG. 5.—Arrangement of Specimens Embedded in Concrete.

had a splice bar 42 in. long, thus affording two 21-in. laps with the main bars, while two had a splice bar 34 in. long, allowing two 17-in. laps with the main bars. Two U-bolt clamps were placed upon each main bar (or 4 clamps upon the entire splice), one clamp 4 in. from the ends of each of the two laps. One of the test specimens with the 42-in. splice, and one with the 34-in. splice, had the splice bar and the two main bars of like twist; one specimen from each length of splice had the two main bars with like twist and the splice bar of the opposite twist.

Specimens Nos. 15 to 20, inclusive, consisted of clamped lap splices, embedded in concrete, with two U-bolt clamps, each one placed 4 in. from the end of the lap. For details of these specimens having the splices embedded in concrete, reference is made to Fig. 5. Two of these specimens had 21-in. laps, the other four only 17 in. One of the 21-in.-lap specimens and two of the 17-in. lap had bars of like twist; while one of the 21-in. lap, and two of the 17-in. lap had bars of the opposite twist. Each such test specimen weighed approximately 1300 lb.

In the preparation of all of the test specimens, and especially of those consisting of splices embedded in concrete, the reinforcement bars were thoroughly cleaned of loose rust by means of a wire brush. The bars were then placed in their proper position and shaken so that the spiral grooves made as snug a fit as possible. The U-bolt clamps were then put in place and the nuts tightened up by means of a wrench with a pipe lever about 2 ft. long. The aim was to secure as firm a splice as possible, but no tighter than could, and should, be duplicated on the work. The general rule applying in fastening the clamps, both upon the actual work and in the preparation of the test specimens, was that the nuts should be screwed until the cross-piece had bent slightly about the reinforcement bars enclosed in the clamp.

Method of Testing.—The tests were conducted in the laboratories of the University of West Virginia. Acknowledgment is made to Prof. C. R. Jones, of the Department of Mechanical Engineering, and Asst. Prof. J. B. Grumbein, of the Department of Experimental Engineering, of the University, for assistance in conducting the tests. An Olsen screw balance

beam machine of 400,000-lb. capacity was used for all the tests, except of course for the cold-bend tests.

In specimens Nos. 15 to 20, inclusive, the concrete was allowed to age for 10 days and to stand undisturbed at the State Line construction site after being poured around the splices. Then the specimens were moved to a locality to permit ease in shipping to the laboratory at Morgantown, West Virginia. They were shipped when the concrete was 25 days old.

Punch marks in all of the specimens with a lap splice (not embedded in concrete) were placed on each of the two bars so as to include the entire length of the lap. These reference

FIG. 6.—The U-Bolt Clamps Removed from Splice after Test of Specimen No. 8.

marks, in the specimens having the lap embedded in concrete, were of course located on the bars extending outside of the concrete.

Upon the butt splices, three reference lengths were taken, (*a*) and (*b*) so as to include each of the two lap splices on each specimen, and (*c*) so as to embrace the distance between two punch marks, one on each of the two main bars. (See Table IV.)

As the load imposed upon each specimen was gradually increased, careful observation was made upon the reference lengths so as to determine the load at first slip of the splice, as well as the slip at the maximum load the splice would stand. By "first slip" is meant the minimum appreciable (approxi-

mately 0.1 in.) movement of one bar past the other; in the case of the butt-splice specimens, the first slip was recorded when observed from either reference lengths (*a*) or (*b*).

Other observations made, of a miscellaneous character, are reported.

RESULTS OF TESTS.

Lap Splices not Embedded in Concrete.—The results of tests of U-bolt clamped lap splices not embedded in concrete are given in Table III.

The two U-bolt clamps removed from specimen No. 8 (Table III) after testing are seen in Fig. 6. A good idea may

TABLE III.—RESULTS OF TESTS ON U-BOLT CLAMPED LAP SPLICES NOT EMBEDDED IN CONCRETE.

ONE-INCH SQUARE COLD-TWISTED BARS.

Specimen No.	Description of Specimen.	Load at First Slip, lb.	Maximum Load, lb.	Slip at Maximum Load, per cent.	Remarks.
7	21-in. lap; like twist.....	9 970	41 470	15.5	Both clamps made markedly loose by test.
8	21-in. lap; opposite twist.....	11 570	40 050	23.0	Specimen twisted in grips through 100 deg.
9	17-in. lap; like twist.....	11 370	43 170	9.9	.....
10	17-in. lap; opposite twist.....	11 260	44 340	12.3	Specimen twisted in grips through 45 deg.

be gathered from this photograph of the strains placed in the clamps upon stressing the splice to the maximum load it would withstand. Also, the bending of the cross-piece of the clamp in the preparation of all of the test specimens may be observed.

In the construction of the Yale Bowl, at New Haven, Conn., 1¼-in. square Havemeyer bars, having a yield point of 55,000 lb. per sq. in., and an ultimate strength of over 64,000 lb. per sq. in., were spliced in a reinforced concrete retaining wall by U-bolt clamps. Preliminary tests were made upon several such clamped lap splices by Mr. Thomas C. Atwood, Supervising Engineer, and Dr. W. K. Shepard, in the Mason Laboratory of the Sheffield Scientific School.[1] A brief summary of the

[1] *Engineering News*, Vol. 73, No. 5, Feb. 4, 1915, p. 218.

results of those tests, wherein the U-bolt clamps were evidently of nearly the same dimensions as the ones used in the tests reported in this paper, is of interest in connection with the study of Table III.

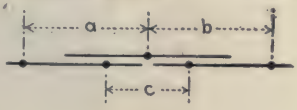
Mr. Atwood and Doctor Shepard made four such tests. In the first two, only one clamp was used on each test specimen. First slip was observed in one case, at 2700 lb., and in the other at 3200 lb. Then, for the third test specimen, two

TABLE IV.—RESULTS OF TESTS ON U-BOLT CLAMPED BUTT SPLICES NOT EMBEDDED IN CONCRETE.

ONE-INCH SQUARE COLD-TWISTED BARS.

Specimen No.	Description of Specimen.	Load at First Slip, lb.	Maximum Load, lb.	Slip at Maximum Load, per cent.[1]	Remarks.
11	42-in. splice bar; like twist....	12 450	31 650	6.6(a) 9.9(b)	Splice bar bent in ½ in. at center, at maximum load.
12	42-in. splice bar; opposite twist	11 210	28 760	5.5(a) 10.0(b) 15.2(c)	Splice bar bent in ¼ in. at center, at maximum load.
13	34-in. splice bar; like twist....	10 100	22 760	14.3(a) 13.3(b) 26.6(c)	Splice bar bent in ½ in. at center, at maximum load.
14	34-in. splice bar; opposite twist	6 950	26 760	12.2(a) 9.5(b) 21.8(c)	Splice bar bent in ¼ in. at center, at maximum load.

[1] The letters (a), (b) and (c) in this column refer to the "reference lengths" marked on each butt splice as in the accompanying figure.



clamps were used, and first slip was observed at 10,000 lb., which agrees very closely with the values given in Table III. For the fourth test, two clamps were used, 16 in. apart, and a ½-in. bar was placed between and at right angles to the reinforcement bars, with the idea that by bending the reinforcement bars around this small bar, a greater resistance to slipping might be obtained. However, first slip occurred at only 6200 lb. It is not stated how tightly the U-bolts were clamped, but merely that a 24-in. wrench was used in fastening them.

Butt Splices not Embedded in Concrete.—The results of tests upon U-bolt clamped butt splices not embedded in concrete are given in Table IV.

Fig. 7 is a photograph of specimen No. 11 (Table IV),

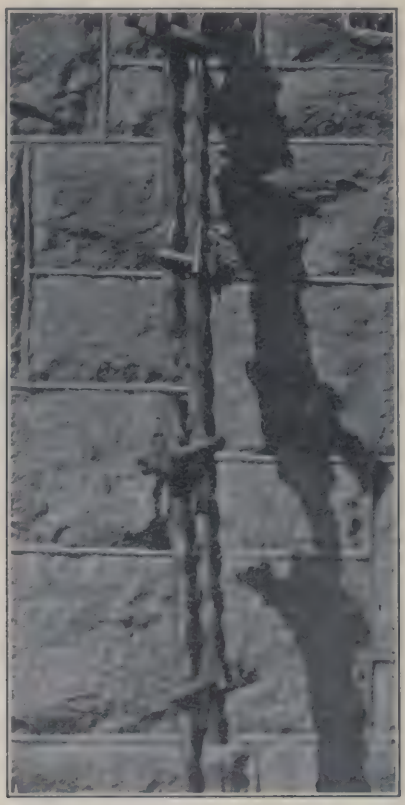


FIG. 7.—A Typical U-Bolt Clamped Butt Splice—Specimen No. 11.

taken after testing. The strained clamps and the bending in of the splice bar are evident.

It is seen, upon comparing Tables III and IV, that while the loads at first slip are approximately the same, the maximum load which the lap splice will withstand is invariably greater than that of the butt splice. Hence, the very intent with which the butt splices were prepared, namely, to obtain higher load

values thereupon than on the lap splices, was not found to be realized. This is perhaps due to the couple formed in the clamped lap splices, the lines of action of the load upon each of the two bars not being in the same straight line. In short, the specimens are eccentrically loaded. Thus, the relatively large frictional resistance due to a turning moment might resist tensile stress, and it would have to be overcome before the failure of the splices occurs.

TABLE V.—RESULTS OF TESTS ON U-BOLT CLAMPED LAP SPLICES EMBEDDED IN CONCRETE.

ONE-INCH SQUARE COLD-TWISTED BARS.

Specimen No.	Description of Specimen.	Age of Concrete, days.	Load, in lb., at				Slip, in Per Cent, at		Remarks.
			First Slip.	First Crack, Top or Bottom.	First Crack on Faces.	Maximum.	First Crack, Top or Bottom.	Maximum Load.	
15	21-in. lap; like twist.....	25	32 000	50 740	60 000	68 950	1.1	4.0	
16	21-in. lap; opposite twist..	26	35 050	49 820	57 180	57 180	2.1	5.0	Specimen twisted through 90 deg.
17	17-in. lap; like twist.....	26	43 200	51 600	58 930	58 950	0.7	3.5	
18	17-in. lap; like twist.....	26	33 000	40 400	52 100	60 960	0.5	7.3	
Av. of Nos. 17 and 18	.....	.....	38 100	46 000	55 515	59 955			
19	17-in. lap; opposite twist..	27	50 000	51 000	64 500	64 000	0.5	4.6	
20	17-in. lap; opposite twist..	27	39 570	39 800	61 940	61 940	1.6	3.2	Specimen twisted through 90 deg.
Av. of Nos. 19 and 20	.....	.....	44 785	50 400	63 220	63 420			

Lap Splices Embedded in Concrete.—Table V gives the results of tests upon the U-bolt clamped lap splices, embedded in concrete.

A series of tests[1] the results of which are comparable with those reported in Tables III and V, was made by the Board of Supervising Engineers in connection with the construction of the walls of the Washington Street Tunnel, Chicago. A brief résumé of those results will be given, because they are

[1] *Engineering and Contracting*, Vol. 38, No. 2, July 10, 1912, p. 38.

the only ones which the writer could find, except those previously mentioned in connection with the construction of the Yale Bowl. Moreover, the tests made by the engineers of the Washington Street Tunnel were the only ones found in which some of the splices were embedded in concrete.

The length of splice in those tests was 8 in. in all cases. All specimens consisted of lap splices, using 1-in. square twisted

FIG. 8.—Splice of Specimen No. 15, Removed from Concrete after Testing.

bars. The clamps are reported as having been tightened sufficiently to bend the cross-piece slightly.

The splice in the first test reported was clamped by two clamps, each having a $\frac{3}{8}$-in. plate. It was not embedded in concrete. First slip ($\frac{1}{8}$ in.) was reported at 6400 lb. and maximum load as 13,690 lb., with a displacement of $\frac{1}{2}$ in.

For the next test the splice was held by two clamps, each of which had a cross-piece $\frac{1}{2}$ in. in thickness. Again, it was not embedded in concrete. First slip ($\frac{1}{16}$ in.) occurred at a

load of 8810 lb., and the maximum load withstood by the splice was 27,000 lb., with a total slip of $2\frac{1}{4}$ in.

Three specimens in which the splices were embedded in concrete were then tested. The U-bolts had $\frac{1}{2}$-in. plates. The concrete was a 1:2:4 mixture of Portland cement, sand, and $\frac{1}{2}$ to $\frac{3}{4}$-in. crushed limestone. The splices were embedded in 12-in. concrete cubes, thus covering the 8-in. lap to a depth of 2 in. at each end. Two tests were made when the concrete was 7 days old. These two tests are reported as having for

FIG. 9.—Portion of Concrete of Specimen No. 18 after Test.

one specimen a first slip ($\frac{1}{8}$ in.) at 23,080 lb. and a maximum load of 31,220 lb., with a displacement of $1\frac{1}{4}$ in., and for the other specimen a first slip ($\frac{1}{16}$ in.) at 19,400 lb., and a maximum load of 34,120 lb., with total slip of $1\frac{1}{3}$ in. A third test was made upon the remaining specimen when the concrete was 28 days old. First slip ($\frac{1}{32}$ in.) occurred when the load was 21,000 lb., and the maximum load reported was 34,480 lb., with a displacement of $\frac{3}{32}$ in.

Tests were also made upon splices clamped with one U-bolt,

and upon splices clamped with three U-bolts. These tests are not summarized here, however, since the results obtained are not comparable with the ones given in Tables III and V of this paper. For use in the walls of the Tunnel, however, splices clamped with two U-bolt clamps were selected.

It is of interest in passing to note one of the conclusions

FIG. 10.—Specimen No. 20, after Test, in the Testing Machine.

reached by the Board of Supervising Engineers, in its report, which is as follows:

"The working stress used in all concrete designs provided for 16,000 lb. per sq. in. in tension in the steel. In these tests the resistance was about 7000 lb. up to the first slip. This resistance, however, would be much greater when the rods were embedded in concrete, so it was decided to use a splice of the type with two clamps and rely on the bonding stress of the concrete to supply the deficiency between the resistance of the splice and the stress in the steel."

It is seen from Tables III, IV and V that in many instances, splices consisting of two bars of opposite twist sustain a greater

load before first slip, and a larger maximum load, than splices in which the bars have like twist. For lack of a large number

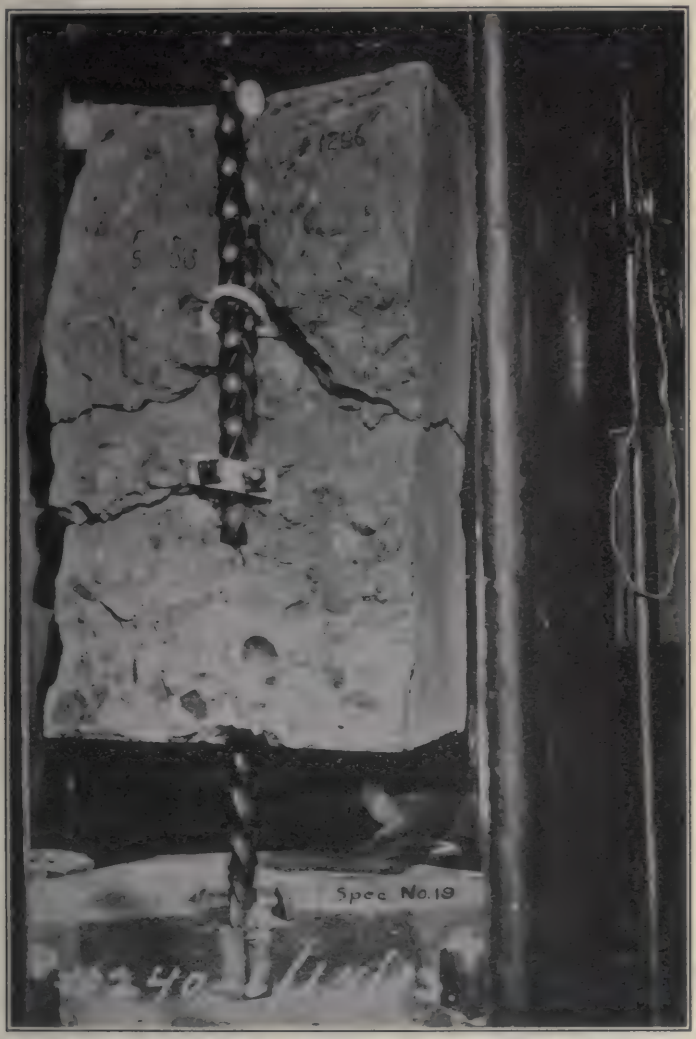


FIG. 11.—Specimen No. 19, after Test, in the Testing Machine, and with the Front or Nearer Half of the Concrete Removed.

of tests, a definite conclusion cannot be drawn, but if this conclusion is warranted, the explanation is perhaps found in the

ability of two bars of opposite twist to fit more snugly together in the first place than bars with like twist; or the reason may be that upon the application of load in the former class of bars, the twisting moment of one bar, as it tends to move past the other bar through the U-bolt clamps, is resisted by the twisting moment of the other bar in its tendency to move in the opposite direction.

Fig. 8 is a photograph of the clamped splice removed

FIG. 12.—Splice of Specimen No. 19, Removed from Concrete after Testing.

from specimen No. 15 (Table V) after testing. It shows in a general way the strains placed in the clamps in testing.

Fig. 9 shows a portion of the concrete from specimen No. 18 (Table V). The character of the concrete and the threaded groove in which the reinforcement bars lay are well indicated.

Fig. 10 shows specimen No. 20 (Table V) in the testing machine, after testing.

Fig. 11 shows a photograph of specimen No. 19 (Table V) in the testing machine, after testing. For this photograph, the nearer or front half of the concrete was removed along cracks caused by testing. The bars and nuts were then painted black, and the clamps themselves and cross-heads white, so as to bring out the details more clearly.

Fig. 12 shows the splice removed from specimen No. 19

TABLE VI.—RELATIVE EFFICIENCY OF U-BOLT CLAMPED SPLICES.

Kind and Condition of Splice.	Specimen No.	Ratio of First Slip to Yield Point of Bar, per cent.	Ratio of Maximum Load to Tensile Strength of Bar, per cent.
LAP-SPLICES NOT EMBEDDED IN CONCRETE:			
21-in. lap	7	16.5	57.1
21-in. lap	8	19.2	55.1
17-in. lap	9	18.9	59.4
17-in. lap	10	18.7	61.0
BUTT SPLICES NOT EMBEDDED IN CONCRETE:			
21-in. lap	11	20.6	43.6
21-in. lap	12	18.6	39.6
17-in. lap	13	16.7	31.3
17-in. lap	14	11.5	36.8
LAP SPLICES EMBEDDED IN CONCRETE:			
21-in. lap	15	53.1	94.9
21-in. lap	16	63.1	78.7
17-in. lap	17	71.6	81.1
17-in. lap	18	54.7	83.9
17-in. lap	19	83.0	89.3
17-in. lap	20	65.6	85.3

(Table V) after testing, and illustrates the same points as those mentioned in describing Fig. 8.

Attention is called to a fact illustrated in Fig. 9, and observed in the case of all of the specimens having the splices embedded in concrete, namely, the spiral groove made in the concrete by the reinforcement bars was not broken by the bar shearing through it. This would seem to indicate that where splices are embedded in masses of concrete larger than any of those of the test specimens, a very much larger tensile stress could be imposed upon the clamped splices before first

slip would occur. That is, in such cases failure by fracturing of the concrete around the splice is not to be expected, and failure by shearing along the threaded groove would only be possible with a greatly increased load. Also the comparatively early age of the concrete at the time of testing must be considered in its effect on the bonding strength. Mr. M. O. Withey[1] has shown that the bond of 1:2:4 concrete to embedded steel increases with age up to at least six months, and that only about 80 per cent of the bond strength at six months is developed at 28 days.

In Table VI are given what are termed relative efficiencies of the U-bolt clamped splices, as compared with the continuous reinforcement bars. These relative efficiencies or proportionate values are expressed both in terms of the first slip to the yield point of continuous bars, and of the maximum load the splice would withstand to the tensile strength of continuous bars. It should be remembered that these results, strictly speaking, apply only to conditions like those obtaining in the tests under discussion. It would seem reasonable to infer, however, that these relative efficiency computations are proximately applicable to clamped splices having a length of lap within a range, say, of between 15 and 25 in. It is also reasonable to believe that they are approximately independent of the size of the bar; that is, that ratios closely approaching those given in Table VI would be obtained if the bars were square, cold-twisted, and of any size within reasonable limits (which limits it is impossible to specify from the tests reported upon). It is probable, however, that other conditions obtaining in the tests under discussion, such as the methods employed in clamping the splices, etc., would have to be duplicated.

Table VII gives a summary of the tests of the various splices.

CONCLUSIONS.

The conclusions which may be drawn from the tests are as follows:

1. The 4-in. difference in lengths of lap tested, of one

[1] "Tests of Bond in Reinforced Concrete Beams," *Proceedings*, Am. Soc. Test. Mats., Vol. VIII, p. 478 (1908).

bar upon the other, apparently does not affect the rigidity or strength of the splice.

2. The clamped lap splices not embedded in concrete sustain, on the average, a slightly greater load before first slip, and a larger maximum load, than the clamped butt splices.

3. Splices consisting of two bars of opposite twist probably sustain a greater load before first slip, and a larger maximum load, than do splices in which the bars have like twist.

4. When U-bolt clamped lap splices (of the type, size, and lengths herein reported upon) are embedded in masses of concrete similar to those of the test specimens, the splices may be expected to withstand a stress before first slip equal to at

TABLE VII.—SUMMARY OF TESTS ON U-BOLT CLAMPED SPLICES.

Kind and Condition of Splice.	Load at First Slip, lb.	Maximum Load, lb.	Slip at Maximum Load in Gage Length, per cent.	Relative Efficiency.	
				Ratio of First Slip to Yield Point of Bar, per cent.	Ratio of Maximum Load to Tensile Strength of Bar, per cent.
Lap; not embedded in concrete	9 970 – 11 570	40 050 – 44 340	9.9 – 23.0	16.5 – 19.2	55.1 – 61.0
Butt; not embedded in concrete	6 950 – 12 450	22 760 – 31 650	5.5 – 26.6	11.5 – 20.6	31.3 – 43.6
Lap; embedded in concrete....	32 000 – 50 000	57 180 – 68 950	3.2 – 7.3	53.1 – 83.0	78.7 – 94.9

NOTE.—The average yield point of the reinforcement bars (1-in. square cold-twisted) was 60,360 lb. per sq. in.; the average tensile strength, 72,685 lb. per sq. in.

least one-half the yield-point stress of the continuous reinforcement bar. Also, the maximum strength of such splices is probably equal to at least three-fourths of the tensile strength of the bar.

5. When U-bolt clamped lap splices (of the type and lengths herein reported upon) are embedded in relatively large masses of concrete, it is reasonable to suppose that first slip would not occur, or the splice would not completely fail, before the yield point or tensile strength, respectively, of the reinforcement steel had been reached; for in such cases the splices would undoubtedly fail only by the pulling out of the bars along the grooves (either through untwisting or through direct shear), and not by splitting of the surrounding concrete. In

either case, unlike the results in the tests, the clamps would remain embedded in the concrete, unless the concrete prism directly compressed by the area of the upper or lower clamps were pulled out also, a condition which is not in the least likely to occur with a relatively large mass of concrete.

6. Hence, for purposes of design, it is probable that such U-bolt clamped lap splices, embedded in concrete under conditions ordinarily obtaining in actual practice, could safely withstand a unit load equal to the allowable unit stress in the steel reinforcement bars.

DISCUSSION.

MR. D. A. ABRAMS.—I have read this paper with considerable interest, but it occurs to me that the writer has overlooked two or three important factors which have a very decided bearing on the interpretation of tests of this kind, and upon the conclusions which may be drawn from the results. Mr. Abrams.

It is stated in the paper that in the clamped splices the first slip is considered to occur at a slip of about 0.1 in. The speaker has made quite a study of the subject of bond between concrete and steel. A large number of tests in which careful measurements of the load-slip relation were made, showed that the maximum resistance of plain or square twisted bars imbedded in concrete is developed at a slip of 0.01 in. and not 0.1 in. The tests referred to above were made on pull-out test pieces; that is, the bar was embedded in a block of concrete and was withdrawn by a tensile stress on one end of the bar. The load-slip measurements were made at the free end of the bar. It would be entirely impracticable to admit a slip of as much as 0.01 in. in a reinforced concrete structure. If a reinforcing bar in a beam slipped as much as 0.1 in., the beam would be entirely destroyed. In fact, it is almost impossible to produce as much slip as 0.01 in. in a beam without destroying the beam. In the case of simple reinforced beams in which the primary cause of failure is by bond, secondary failure by diagonal tension or by splitting or stripping of the concrete occurs immediately after first end slip of the reinforcing bars is observed.

Another factor that seems to have been overlooked is that twisted square bars do not shear out the concrete that surrounds them, but untwist as they are withdrawn from their embedded position. It will be readily appreciated that a piece of cold-twisted steel is in an unstable condition; it takes a very small stress to cause this twist to begin to unwind. That is what actually occurs in cold-twisted square bars as they are pulled out of concrete. The tests referred to above are discussed in

Mr. Abrams. "Tests of Bond between Concrete and Steel," *Bulletin No. 71*, Engineering Experiment Station, University of Illinois.

The author would have secured a great deal more information concerning this point if he had made parallel tests on bars of the same kind embedded in the same concrete, which were not clamped together. I am thoroughly convinced that he would have found almost exactly the same results. The illustrations in the paper show that in order to develop these high stresses, at the splice, a very large movement of the bars must occur. The clamps must be pulled aside, at an angle so that they take a component of the longitudinal stress, before they can have any appreciable effect. When a splice of this kind is embedded in concrete, the clamp cannot be pulled aside; consequently it can offer very little resistance to a bar of this kind slipping through. I am willing to grant that there may be an infinitesimal resistance due to the clamps; but when it comes to transmitting any considerable part of the stress from one bar to another, I believe it is an absolute fallacy.

One wonders why square twisted bars are used in reinforced concrete work. The tests made by the speaker have clearly shown that twisted square bars are less effective, so far as load resistance is concerned, than plain round bars.

Mr. Collings. MR. W. A. COLLINGS (*by letter*).—It seems strange that, in view of the present knowledge of bond between concrete and steel, there should have been thought to be any necessity for making tests of the kind described.

The paper mentions "the meagerness of published data" and again the "scantness of available data," in apparent disregard of the up-to-date investigation reported in *Bulletin No. 71*, University of Illinois, "Tests of Bond between Concrete and Steel," by Prof. D. A. Abrams under the supervision of Prof. Arthur N. Talbot.

This bulletin covers tests of bond made at the University from the year 1909 up to the date of the bulletin (1913), and contains 238 pages of most valuable information relating to an extended range of tests for bond.

Referring briefly to the author's conclusions, the highest bond values he found with U-bolt splices reached about "three-fourths of the tensile strength of the bar." Of what consequence

is this result when any good type of deformed bar on the market to-day will develop the full elastic limit of the steel if imbedded in the concrete to a depth of 30 times the diameter of the rods. Mr. Collings.

Professor Talbot recently made some tests on bond, for the writer, on an ordinary style of deformed bar with projections at right angles to its axis. The sections used were $\frac{1}{2}$, $\frac{3}{4}$ and 1 in. rounds and 1 in. squares. In 16 out of a total number of 25 tests, these bars developed the full elastic limit of the steel when imbedded in the concrete to a depth of only 8 in., that is to say, from 8 to 16 times the diameter of the bar, the concrete failing in every case.

The fact that the bars, when imbedded in the concrete with the clamps on, gave higher values than when they were tested out of the concrete, shows, I believe, that the mechanical bond of the twisted square was doing more than half the work of preventing the rods from slipping in the concrete.

Another disadvantage of the plan used by the author is to be found in the rods lying so close together that the concrete could not envelope them. In this way, probably 25 per cent of the natural bond value of the bar was lost.

A further disadvantage lies in the cost of the clamps and the added labor of applying them on the work. This additional cost alone should serve, I believe, to condemn the scheme. Evidently the aim in all such matters should be to simplify construction and to reduce cost, instead of adding complications and increasing expense.

From the practical concrete engineer's standpoint I can see nothing of value in these clamp splices, because better bond values may be gotten from ordinary deformed bars, and without the additional labor and material cost necessary under Mr. Lasier's plan.

MR. E. L. LASIER (*Author's closure by letter*).—Mr. Abrams' point concerning the practicability of 0.1 in. slip in a reinforced concrete beam is well taken. The integrity of such a structural member would certainly be lost if a slip of that magnitude occurred. Undoubtedly the experience of Mr. Abrams makes his value of 0.01 in. (or even less) slip correct. A reinforced concrete beam, however, presents conditions entirely different from those present in the test specimens tested, and an allowable Mr. Lasier.

Mr. Lasier. slip in a beam must not be assumed as being the same as for the blocks tested.

The concrete of the test specimens, Nos. 15 to 20 inclusive, did not fail before the occurrence of first slip of the reinforcement. Table V gives values not only of "load in pounds at first slip," but also of load at first visible crack on the top or bottom of concrete, and load at first visible crack on the faces of concrete. In every case it is seen that the order of their occurrence was: first slip of bars, then first crack on top or bottom, then first crack on faces. The first crack, top or bottom of concrete, always occurred, as might be expected, close around the projecting reinforcement bar.

An item that was not brought out in the paper, except by the definition of first slip as "the minimum appreciable (approximately 0.1 in.) movement of one bar past the other," was that the first slip in inches was very variable; probably it would have been better to have stated in the paper that 0.1 in. was about a maximum. The slip seemed to occur suddenly, all at once, and amounted after occurrence to from 0.02 or 0.03 up to 0.10 in. It was in fact, as stated, the "minimum appreciable" movement of one bar past the other.

The noted sudden occurrence undoubtedly meant that the increasing load forced all at once an adjustment of the clamps so that a fuller bearing was obtained than was possible to obtain in splicing and clamping by hand methods. In other words, what occurred was probably what Mr. Abrams well describes as the pulling aside of the clamps at angles so that they could take components of the longitudinal stress.

The same conditions of sudden slipping, all at once, were probably encountered in the tests[1] made by the Board of Supervising Engineers in connection with the construction of the walls of the Washington Street Tunnel, Chicago, and reported, in abstract, in the paper. There the first slip, in the tests abstracted, varied from $\frac{1}{32}$ to $\frac{1}{8}$ in.

Mr. Abrams and Mr. Collings both mention tests made by the former at the University of Illinois. An interesting and important point in connection with the discussion on the measurements of slip is gleaned from some of the bond stress-slip curves

[1] *Engineering and Contracting*, Vol. 38, No. 2, July 10, 1912, p. 38.

Mr. Lasier.

given as Figs. 23 and 24 obtained from Mr. Abrams' pull-out tests.[1] The curves in question show a peculiarity for 1-in. square twisted bars different from the other types of deformed bars tested. Up to approximately 345 lb. per sq. in. bond stress, the slip for 1-in. square twisted bars was only 0.01 in.; with the slip increasing to 0.05 in. or even slightly larger, the stress remained constant,—indeed the increase of bond stress between slips of 0.01 and 0.10 in. was only approximately 55 lb. per sq. in. In other words, judging from Mr. Abrams' pull-out tests, even though his tests were not on clamped splices, it can be inferred that the values of load at first slip obtained by the writer were little, if any, in excess of values of load at a slip of 0.01 or 0.02 in., had such load values been obtained.

As stated in the paper, every attempt was made (without taking any more care than could and should be duplicated in actual construction) to shake the reinforcement bars into such place that the spiral grooves made as snug a fit as possible.

The untwisting of square twisted bars upon application of load—a fact called attention to by Mr. Abrams—was borne out by the turning in the testing machine of the embedded lap spliced specimens tested, two of which, as reported in Table V, twisted through 90°. It would appear that this characteristic of twisted bars is important and should be investigated further; the instability, if such it be, of square twisted bars can be dangerous.

Mr. Collings seems to have been under the impression that the tests made were for the purpose of obtaining further bond resistance data. The writer believes that the title of the paper, as well as the subject matter therein, adequately shows that desire for further knowledge of bond resistance alone was furthest from the minds of the engineers conceiving the tests. Of course, bond plays a large part in the strength of clamped splices. In lieu of additional tests, the writer cannot agree, however, with the implied statements of either Mr. Collings or Mr. Abrams that if test specimens similar in every way to specimens Nos. 15 to 20 inclusive, except that the clamps were omitted, had been prepared and tested they would have withstood loads equaling

[1] "Tests of Bond between Concrete and Steel," *Bulletin No. 71*, Engineering Experiment Station, University of Illinois, issued December 8, 1913, pp. 70–71.

Mr. Lasier. those reported in Table V. Mr. Collings' remark regarding loss in bond value because the reinforcement bars lay in contact with each other is pertinent and correct, although whether such loss would amount to 25 per cent or not is a question.

As to the added cost of labor and clamps compared to the cost of carefully setting and keeping in place unclamped reinforcement, the real question at issue is this: Is the cost of clamps and additional labor greater (if it be greater) than the cost of additional length of steel required for a lap insuring by bond alone the same factor of safety? The average value of load at first slip obtained for the splices tested, such splices being embedded in comparatively small masses of concrete, was 39,300 lb., with a minimum value obtained of 32,000 lb. and a maximum of 50,000 lb. That is, assuming an allowable fiber stress in the reinforcement of 16,000 lb. per sq. in., the factors of safety, based on the values of load at first slip for the tests made and reported upon, had as an average 2.5, and minimum and maximum values of 2.0 and 3.1 respectively. To obtain those same factors of safety by bond resistance alone, a lap of at least 50 diameters would probably be necessary as against the lap of less than one half that used in the test specimens. Then, too, it must be remembered that in certain types of reinforced concrete construction it is highly desirable, if not necessary, to use as short a length of lap as possible.

The writer must not be understood to favor clamped splices for all work; the purpose of the paper and this closure, as well as the purpose of the gentlemen who by their discussions gave of their experience, was to present information for the further study by and use of engineers. The writer is aware that there is a fairly prevalent feeling among a great many reinforced concrete designing engineers that reinforcement bars should be lapped, when necessary, over a length sufficient to develop by bond alone the required strength, without reliance upon clamps, clips, couplings, welds, or other gripping devices. The divergence of opinion on the question is, however, evinced by the series of opinions of several well-known engineers recently appearing in the technical press.[1]

[1] For instance, *Concrete*, Vol. 8, No. 6, June, 1916, pp. 273–274.

THE ACCEPTABILITY OF LINSEED OIL.

By C. D. Holley.

SUMMARY.

The object of this paper is to show the necessity of bringing the linseed oil producer, the paint manufacturer and the paint user together on a basis of common agreement on the question of the acceptability of "foots" in linseed oil.

A method is also presented for distinguishing a properly aged oil from an unaged oil by treatment with phosphoric acid and making a comparative reading of the volume of foots found after diluting with naphtha and allowing to stand for a specified interval.

Comparative results are given for a number of variously aged linseed oils. A properly aged oil will show from a trace to 0.3 per cent, while an unaged oil may show as much as 5 per cent of foots.

THE ACCEPTABILITY OF LINSEED OIL.

By C. D. Holley.

The Specifications for the Purity of Raw Linseed Oil (Serial Designation: D 1–15),[1] as adopted by the American Society for Testing Materials, relate to the purity of the oil only, and are not intended to cover the question of quality, as this phase has not as yet received the final consideration of the linseed oil committee. The writer believes that the further investigation of this problem is not only highly desirable but very necessary, as the question of quality is at the bottom of nearly all the trouble the paint manufacturer has in securing acceptable linseed oil for use in paints for public service corporations and the various railroad systems. The thoroughness with which these corporate interests examine their paint purchases is increasing yearly, and the gap between what the paint manufacturer is obliged to accept as raw linseed oil and what the various corporations will permit to go into the paint used by them is increasing each year.

The basis of this trouble is the condition of the oil as received by the paint manufacturer and contracting painter, as regards lack of age and the frequent presence of large quantities of "foots." A large producer of linseed oil recently admitted to the writer that his normal procedure was to ship the oil as soon as it left the filter presses, no time being allowed for settling or aging, and a comparison of oils from several producers indicate that this manufacturer was not alone in following this practice.

The writer does not wish to be understood as stating that serviceable paint for many purposes cannot be made from oil containing an appreciable amount of foots, for he knows that such paints are made daily. What the writer particularly wishes to emphasize is, that the paint manufacturer is not permitted to sell such paints to the leading railroad, public service and other corporations, who purchase according to specifications.

[1] 1915 Year-Book, p. 417.

The following specification requirements are typical of an almost innumerable number that the paint manufacturer is called upon to meet. The New York, New Haven & Hartford Railroad Co. requires that the oil used in their paints must "have a pale yellow color . . . be well clarified by settling and age . . . must not contain more than 1 per cent of foots." Similarly, the Southern Pacific Railroad Co. requires "oil of a pale yellow color, well clarified by settling and age. . . . It must contain less than 1 per cent of foots by volume when 100 cc. is allowed to settle in a graduated cylinder for 24 hours at room temperature." The Baltimore & Ohio Railroad Co. specifications require that the "oil must contain less than 1 per cent foots."

The Atchison, Topeka & Santa Fé are more stringent in their requirements: "the linseed oil must be pure raw linseed oil free from foots." The Pennsylvania Lines require that the oil must be "as free as possible from foots and well clarified by settling and age." The Michigan Central requires among many other things that the oil used "shall be drawn from settling tanks in such manner that the foots and other albuminous matter contained in it shall not exceed 1 per cent."

The Navy Department requires that the oil used shall "be well-settled linseed oil, perfectly clear, and not show any deposit of foots when heated for one-half hour to a temperature of 103 to 105° C." Likewise the purchase specifications of the Quartermaster Corps, U. S. Army, require that the oil used must be perfectly clear at 60° F. and not show any deposit of foots when heated to 212° F.

The City of New York specifications state that the oil used in the paints purchased shall be heated to 65° C. and allowed to stand in a graduated cylinder for 36 hours at a temperature not lower than 20° C., and that not over 2 per cent of the material shall separate out. The City of Pittsburgh adheres to the Navy Department specifications as given above.

A large producer of white lead in oil, in order to meet the requirements of the trade, has found it advisable to supply their own oil put up in sealed packages and lay particular emphasis in their circulars that this "linseed oil is clear, bright, well settled and free from foots."

Not only is the paint manufacturer barred from using oil containing an appreciable amount of foots in corporation paints, but the recent investigations of Gardner on house paints are causing various state paint and oil officials to regard the question of foots in a serious manner, as indicated by the following excerpt from the Oil Report for 1915, Pennsylvania Department of Agriculture, *Bulletin 274*, page 10: "According to H. A. Gardner, excessive amounts of mucilaginous and albuminous matter contained in linseed oil, commonly called foots, is unfit for painting purposes, as these foots usually contain micro-organisms which . . . affect the life of the paints in which such oils are used." Also the experiences of several of the prominent contracting painters with "foots oils" lend further weight to the seriousness of the situation and the necessity of seeking a suitable means that will bring the oil producer, the paint manufacturer and the paint user together on a basis of common agreement.

The receipt of several shipments during the past year of raw oil containing abnormally large quantities of foots, with the factory and sales troubles resulting therefrom, caused the writer to seek a suitable method of comparing the relative amounts of foots in different raw oils. Nothing was found in the chemical literature that was of material assistance and the measurement schemes as given in the various specifications already referred to were not sufficiently definite to be satisfactory. Finally the following procedure of treatment of the oil with phosphoric acid was developed, and gave results of practical value for comparing the foots present in various oils and especially for determining whether the oil was aged sufficiently to stand inspection in specification paints:

One hundred cubic centimeters of the linseed oil, well shaken, are introduced into a short small-neck 500-cc. flask, brought to a temperature of 30° C., 100 to 105 cc. syrupy phosphoric acid (sp. gr., 1.710 at 15° C., equal to 85 per cent acid) previously brought to 30° C. are added and the mixture thoroughly agitated for 1 minute, allowed to stand for 1 minute and 150 cc. of petroleum naphtha (sp. gr. 0.755 to 0.758) added and the contents of the flask again agitated for 1 minute. The flask is then connected by a short rubber tube (approximately 2 in. long) to a flask of 110-cc. capacity, the neck of which is graduated in tenths

of a cubic centimeter, and the larger flask supported in an inverted position. The phosphoric acid collects in the lower or graduated flask, the foots forming a definite layer on top of the acid in the

TABLE I.—COMPARISON OF AGED AND UNAGED LINSEED OILS.

Oil No.	Kind of Oil.	Volume of Foots, Percentage in 3 hr.	Volume of Foots, Percentage over Night.
1	Direct from filter press	3.9	2.7
2	" " "	4.0	3.2
3	Aged 4 weeks at 70° F.	0.2	0.2
4	" 4 " " "	0.3	0.2
5	" 6 " " "	0.1	0.1
6	" 8 " " "	0.2	0.2
7	" 8 " " "	0.1	0.1
8	" 3 months " "	0.2	0.1
9	" 6 " " "	0.1	0.1
10	" 6 " " "	0.2	0.2

TABLE II.—TESTS OF OIL SHIPMENTS AS RECEIVED.

Oil No.	Kind of Oil.	Volume of Foots, Percentage in 3 hr.	Volume of Foots, Percentage over Night.
1	Raw Linseed Oil	1.0	0.6
2	" " "	3.2	2.8
3	" " "	3.0	2.6
4	" " "	0.4	0.3
5	" " "	0.9	0.8
6	" " "	1.1	0.9
7	" " "	2.8	2.5
8	" " "	4.3	3.9
9	" " "	4.6	4.0
10	" " "	3.6	3.3

graduated neck. The temperature should be maintained between 25 and 30° C. A preliminary reading may be made at the end of 1 hour and the final readings at the end of 3 hours and after standing over night.

At the prescribed temperature and with the stated strength of acid, the foots easily collect in a compact mass. Lower temperatures and weaker concentrations of the acid cause trouble in the separation and retard the gathering of the foots into compact masses. Under uniform conditions of operation, an oil direct from the filter press will give a volume reading of 3 per cent and upwards, while a tanked oil, suitably aged, gives only 0.1 to 0.3 per cent.

Table I gives a comparison of aged and unaged oils.

Table II gives results of tests on oil shipments as received. Using lampblack as the pigment two paints of brushing consistency were prepared, one with oil No. 9 and the other a well-aged oil for the vehicle. The paints were brushed as evenly as possible on sheets of carefully cleaned glass and allowed to dry at room temperature. The paint made with the aged oil required 7 days to dry, while the paint made with oil No. 9 took 9 days.

TABLE III.—EFFECT OF AGING ON VOLUME OF FOOTS.

Oil No.	Time of Aging at Approximately 70° F.	Volume of Foots, Percentage as Received.	Volume of Foots, Percentage after Aging.
1	6 weeks	2.1	0.3
2	8 "	1.8	0.2
3	8 "	1.6	0.2
4	3 months	1.1	0.1
5	3 "	0.8	0.2
6	3 "	0.7	0.1
7	4 "	3.0	0.3
8	4 "	1.3	0.1
9	5 "	2.1	0.2
10	6 "	0.9	0.2
11	6 "	2.5	0.1
12	8 "	1.4	0.2

Table III shows the effect of aging on the volume of foots. After having been aged at room temperature (70° F.) as stated, oils Nos. 1, 3 and 8 were chilled to −15° C. for several hours. When brought to room temperature again, they were perfectly clear and remained clear after having been heated to 103° C.

There may be raw oils that are difficult to classify by means of this test, but no such oils have been obtained so far by the writer and those already examined represent numerous shipments from all of the leading crushers.

The writer regards this as a comparative method only, for determining whether a raw linseed oil is well clarified and suitably aged or not, and for the detection of abnormal quantities of foots and a relative means of numerically expressing the amount present. The problem of just where to draw the line of demarcation as to the amount of foots allowable, is one that demands careful and extended investigation and cannot be answered in this paper, which aims to deal with the subject in a preliminary way only and to point out the difficulties that confront the paint manufacturer in marketing his products.

DISCUSSION.

Mr. Pickard. MR. G. H. PICKARD.—I should like to say, on behalf of Sub-Committee V on Linseed Oil, of Committee D-1, that it has not been unmindful of the condition which Mr. Holley sets before us. We had outlined for this year the development of a test which, while not the same as that used by Mr. Holley, would bring about the same results. The matter was in the Chairman's hands, but while working on the test, the laboratory in which he worked was destroyed by fire and the matter therefore had to be dropped.

The amount of footy material present in linseed oil, as controlled at least by the aging, is due to the relation of supply to demand. When business is brisk, it is absolutely impossible for any length of time to be given to the storage of oil; buyers would not stand for it. The footy material to which the author refers is of uncertain composition. It often contains a large percentage of high-melting-point fats which crystallize out very readily. These are present in varying amounts, depending on climatic conditions during the maturing of the seed and on other conditions over which the producer of linseed oil has no control. While the matter is recognized as vital, it is a part of the production of linseed oil over which control, without additional expense, is impossible.

Mr. Gardner. MR. H. A. GARDNER.—Mr. Holley's paper is an important and timely one, since the subject of "foots" in linseed oil has not been given sufficient consideration in the past. A year ago I was called to Philadelphia by several master painters to investigate the conditions shown by fifteen or twenty dwellings in Germantown that had been painted white. The paint had apparently been affected by the weather to a great extent, although it had been applied only a few months. By moistening the paint, it could be easily rubbed off, presenting a rather soapy condition. At the bottom of columns, the pigment could be seen piled up, where the rains had deposited it after removing portions of the paint. In nearly every instance the white lead

that was used, or the mixture of white lead and other pigments, had been mixed with a shipment of linseed oil that proved to be very high in foots. Samples of the oil had in some cases been reserved, and I examined them carefully. They contained on an average 15 per cent by volume of foots, after settling for 24 hours. These foots contained a very large percentage of moisture. It is quite probable that when such oil is used in a paint, the paint does not dry properly. If damp weather follows, absorption of large quantities of moisture from the air may cause the formation of an emulsion-like film which may be readily washed off by heavy rains. Probably it would be well for the painter to examine samples of oil before using them. If such samples are placed in tall glass cylinders and allowed to stand for 24 hours, they should not show more than 2 per cent by volume of precipitated foots, if of the highest quality. Reduction of the oil with an equal or greater quantity of benzine may hasten this precipitation. The foots in linseed oil consist of albumen and mineral matter such as silica, clay and calcium phosphates. The mineral matter is probably held in suspension as a suspended colloid. By heating the oil, the albumen may be coagulated, bringing down with it the colloidal mineral matter. That this matter is present as a colloid, I have found by examining under the ultramicroscope perfectly clear samples of raw oil, intense Brownian movement being observed if the oil is diluted previously with a disperse-free medium such as benzol. Mr. Gardner.

Often crushers will furnish the consumer linseed oil fresh from the press. If the crusher would heat such oil to 100° C., in order to coagulate the albumen, and then allow it to settle, or properly centrifuge the oil, a clear product would be obtained, practically free from foots. I am sure that such oil would give much better painting results than ordinary raw oil.

QUANTITATIVE TEST FOR RESISTANCE OF LUBRICATING OILS TO EMULSIFICATION.

By Winslow H. Herschel.

SUMMARY.

With forced-feed lubrication, an oil will not pass through the filter after it emulsifies, and it has to be cleaned out of the oiling system. To avoid this annoyance it is desirable to have a laboratory test by which non-emulsifying oils may be selected. There is no quantitative test in general use. The new test, which is the subject of this paper, is made as follows:

Twenty cubic centimeters of oil and 40 cc. of distilled water are placed in a 100-cc. cylinder having an inside diameter of 26 mm. (1 in.), and heated in a water bath to 55° C. (130° F.). The oil and water are then stirred with a paddle for 5 minutes at a speed of 1500 r.p.m. The paddle is a flat metal plate, 89 by 20 by 1.5 mm. ($3\frac{1}{2}$ by $\frac{13}{16}$ by $\frac{1}{16}$ in.) conveniently driven by an electric motor. The mixture is allowed to stand at a temperature of 55° C., and readings taken at frequent intervals of the volume of clear oil settled out from the emulsion. The rate of settling, in cubic centimeters per hour, is readily found in Table III. There is a tendency for the rate of settling to increase up to a maximum, and then decrease. The maximum rate is called the "demulsibility" of the oil. If readings are taken to even minutes and even cubic centimeters, the highest possible demulsibility is 1200.

The best transformer, dynamo and turbine oils settle out in a minute or less, thus having a demulsibility of 1200. Oils are on the market with rates of settling all the way down from this maximum to zero. From experience of a year and a half with this test it is believed that if oils are purchased by specifications, in which a suitable value for the demulsibility is stated, there will be no trouble from emulsification.

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The most usual method of applying oil in the case of high-speed machinery is by means of a circulatory system, the oil being forced around a closed circuit, through an included filter, and used over and over again. In order to reduce the danger of the oil becoming unserviceable on account of chemical changes, it has been found advisable to use only pure mineral oils, free from acid or paraffin, but the idea, once prevalent, that oils "wear out," in a mechanical sense, has been generally abandoned. Thurston[1] says:

"A mineral oil is usually just as good after use as before, apart from the impurities, which are removed in filtering."

This subject has been investigated also by Sabatié and Pellet[2] who conclude:

"The apparent result of all these different tests is that a used oil, received in good condition and *filtered with care* [their italics] to rid it of the material which it may contain in suspension, preserves its different properties almost intact. In fact most manufacturers are already aware of this fact, since many of them have collected and regenerated their oils for some time; but we have thought it of interest to determine exactly the modifications produced in certain cases, and our observations are entirely in accord with the usual experience."

The more or less solid substances which form after *long-continued use* are due to chemical changes in the oil which are not considered.

In the case of high-speed engines and steam turbines, there is almost sure to be some leakage of water, either from stuffing boxes or cooling coils. Hence one of the most prolific causes of trouble is the formation of emulsions which clog the filter and must be cleaned out of the oiling system at considerable trouble and expense. It is therefore highly desirable to employ a

[1] "A Treatise on Friction and Lost Work in Machinery and Millwork," 1903, p. 380.

[2] *Bulletin du Laboratoire d'Essais*, No. 16, 1911; "Essais d'huiles Usagées," P. Sabatié and M. Pellet.

laboratory test to determine the "demulsibility," or resistance of an oil to emulsification, so that service tests may be dispensed with.

It is recognized that the tendency of an oil to emulsify may be increased by the addition of animal or vegetable oils, which may improve the oil in other respects. But fixed oils are too expensive to be used as an adulterant, and are not usually desired, except in the case of steam-engine cylinder oils. We shall therefore consider that one is dealing with pure mineral oils only, and shall speak of a non-emulsifying oil as "better" than one which emulsifies.

TESTS PREVIOUSLY EMPLOYED.

A common test for emulsibility is to pass steam over or through the oil.[1] This method does not seem very promising, on account of the difficulty of measuring the steam quality, and reproducing this and other conditions. Archbutt and Deeley[2] propose a shaking test which they describe as follows:

"*Emulsification Test.*—Oils used for forced lubrication should separate quickly and completely from water. To test this property, equal volumes (say 50 cc. each) of the oil and water should be heated to 80° C. or 180° F. and vigorously shaken together for 10 minutes in a corked bottle about 2 in. in internal diameter. Some mineral oils will, on allowing the bottle to stand, separate from the water rapidly and completely, others will form an emulsion which breaks up quite slowly."

A similar test, which is somewhat more definite, is given by Stratford,[3] who says:

"Fill a bottle (preferably 4 oz.) one-third full with the oil to be tested. Pour in an equal amount of water, leaving a space of one-third free above the oil and water. Cork and shake the bottle vigorously 30 minutes in a shaking machine or by hand. Then set it aside for 24 hours."

Bancroft[4] has compiled a great amount of information about emulsions, but unfortunately for our purpose, the experimenters

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[1] Conradson, *Journal of Industrial and Engineering Chemistry*, May, 1910, p. 171; G. S. Bryan, *Journal*, Am. Soc. Naval Engrs., Vol. 26, p. 561 (1914).

[2] "Lubrication and Lubricants," p. 343.

[3] Paper presented before Soc. Auto. Engrs., June, 1915. The illustration shows a milk shaker modified to hold two 4-oz. bottles.

[4] *Journal of Physical Chemistry*, 1912, pp. 177, 345, 475, 739; 1913, p. 501.

quoted were dealing mainly with fixed oils and permanent emulsions. Speaking of Ostwald's experiments, Bancroft[1] says:

"The oil, or water as the case might be, was placed in a tall glass cylinder and the other liquid emulsified in it gradually by means of an electrically driven stirrer. In this way a snow-white emulsion of kerosene and water was obtained which began to clear, on standing, at the top and at the bottom. When everything was clean, it was possible to note two fairly sharp meniscuses; the rate of movement of each meniscus was noted and plotted on coordinate paper."

Ostwald's apparatus might be described as a "churn" since his paddle had a reciprocating motion,[2] but as he does not give the length of stroke nor the dimensions of his paddle, his apparatus could not be reconstructed.

Bryan[3] also fails to describe the paddle used in his stirring test, which differed however from that of Ostwald in having a rotary motion. Speaking of mixtures of half water and half oil, he says:

"These mixtures were stirred in a pint commercial fruit jar for five minutes, and were then poured in graduated glass cylinders and allowed to settle out at room temperature (70 to 80° F.), observations being made periodically of the total amount of oil and water that had not separated from the mixture."

Finally we would refer to Philip,[4] who likewise used a rotary stirrer, and whose test is the only one discovered which gives a numerical value for the resistance of an oil to emulsification. With the exception of the paddle, the dimensions of which are not given, the apparatus and method of making the test are fairly well described.

The speed of the paddle is 350 to 400 r.p.m., temperature 100° C., and the mixture of 500 cc. of distilled water and 500 cc. of oil is stirred for five minutes and then run off from the tester into a previously warmed 1000-cc. graduated glass jar, and set aside to stand. At the end of 24 hours the percentage of oil which has separated out, calculated on the volume of oil originally placed in the tester, is recorded as the "demulsification

[1] *Journa. of Physical Chemistry*, 1912, p. 180.
[2] Ostwald, *Zeitschrift für Kolloidchemie*, Vol. 6, p. 103 (1911).
[3] *Journal*, Am. Soc. Naval Engrs., Vol. 26, p. 559 (1914).
[4] *Journal*, Soc. Chem. Ind., July 15, 1915, p. 697.

value." This is a measure of the power of the oil to resist emulsification.

REQUIREMENTS OF A STANDARD TEST.

A consideration of the various tests which have been used led the author to adopt the rotary stirrer as the most promising method. In devising a test on this basis, the following requirements were kept in mind:

1. The apparatus should be reproducable and readily constructed, and capable of operation by any power-house engineer.

2. The test should require the minimum amount of time and material consistent with the desired degree of accuracy.

3. The resistance to emulsification should be expressible by a numerical value, ranging from a minimum reached by but few of the worst oils, to a maximum attained by only a few of the best.

FEATURES OF THE TEST.

Having decided on the general type of machine, there remained a number of details to be settled, both in regard to the apparatus and its method of operation, and these will be considered in order.

Oil Container.—Both Bryan and Philip stir the oil and water in one vessel and pour it into another, with consequent drainage losses. Philip speaks of "three occasions upon which the oil and water are transferred from one vessel to another," and says, "one of the results of the drainage losses in transference is that even an ideal oil can not give a demulsification value greater than about 98 per cent." On this account it appeared preferable to stir the oil in the graduated cylinder itself, so that the only possible drainage loss would be that due to the emulsion clinging to the paddle when it was withdrawn; a loss easily reduced to a negligible amount by wiping the paddle with the finger, or other non-absorbent body, and returning to the cylinder the emulsion thus collected.

In selecting the size of cylinder, the following additional considerations were kept in mind:

1. The error due to taking readings to even cubic centimeters should not be excessive;

2. The liquid should not overflow when stirred at high speed; and

3. The cylinder should be of a readily obtainable size.

The 100-cc. cylinder graduated from the bottom seemed to fill all requirements and was adopted. It was subsequently noted that this size of cylinder is shown in the illustrations in Ostwald's article.

Paddle.—Any stirrer similar to a screw propeller would be difficult to describe or to reproduce. If it were not for this restriction, a Dover eggbeater, familiar in many households, might have been adopted, as it is an efficient emulsifier. But for the sake of simplicity it was decided to use a rectangular

TABLE I.—VARIATION OF SEPARATED MATTER WITH TEMPERATURE.

Temperature at which mixed.		Separated Matter, per cent.
deg. Fahr.	deg. Cent.	
51	10.6	1
65	18.3	3
83	28.3	5
120	48.9	26

plate which could be completely described by giving its length, breadth and thickness, and to rely on a narrow clearance between the edges of the paddle and the cylinder walls to produce the emulsifying effect. It was found that 60 cc. of liquid, filling the cylinder to a height of about 10 cm. (4.25 in.) was an amount that could be used without danger of spilling. With this amount, and taking into consideration the necessity of keeping the paddle submerged, so as to avoid the formation of foam, it appeared that 89 mm. ($3\frac{1}{2}$ in.) would be a satisfactory length of paddle. As the inside diameter of the cylinder was found to be 26 mm. (1 in.), the width of paddle was taken as 22 mm. ($\frac{7}{8}$ in.), giving a clearance of 2 mm. ($\frac{1}{16}$ in.) at each side, which is about the minimum value that could be used without the necessity of having a very solid instrument made by an instrument maker. The thickness was taken as 1 mm. ($\frac{1}{32}$ in.). A width of 20 mm. ($\frac{13}{16}$ in.) and thickness of 1.5 mm. ($\frac{1}{16}$ in.) were afterwards adopted to increase the clearance and prevent danger of bending the

paddle when wiping it. The change of paddle produced no appreciable change in the value for demulsibility.

Temperature.—Bryan[1] recognized that the temperature at which the oil and water were mixed, as well as the temperature of the mixture while settling, affected the results. He gives the data shown in Table I, showing variation in the amount of "separated matter," a term which includes everything except clear water and oil which has separated out.

The writer adopted a temperature of 130° F. (55° C.), both for stirring and settling. This temperature possesses the following advantages:

1. There is no danger of breaking the cylinders from temperature changes;

2. The temperature is sure to be above room temperature, and it is convenient that it can always be reached without the use of ice;

3. This is approximately the running temperature of a bearing; and

4. This is one of the standard temperatures for viscosity tests, recommended by Committee D-2 on Lubricants of the Society.[2]

Duration.—Bryan and Philip used five minutes as the length of time to agitate the liquid. Bancroft[3] quotes Scoville as follows:

> "Five minutes of very rapid trituration will accomplish more in emulsie fying an oil or balsam than an hour of slow trituration. Indeed, if after fiv-minutes or less of rapid trituration an oil does not emulsify, it is good evidence of some fault in the ingredients used or of their proportions, and a satisfactory emulsion cannot be expected without alteration of the ingredients."

In reading this quotation it should be remembered that, from the point of view of the pharmacist, only a permanent emulsion is "satisfactory." As five minutes did not seem an inconvenient length of time, it was adopted.

Speed.—But few precedents could be found for any particular speed, although the above quotation would indicate that high speed is desirable. Bryan turned his stirrer by hand; Ostwald and Stratford used electric motors. If an electric

[1] *Journal*, Am. Soc. Naval Engrs., Vol. 26, p. 561 (1914).
[2] *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part I, p. 279 (1915).
[3] *Journal of Physical Chemistry*, 1912, p. 756, from "The Art of Compounding," 1895, p. 81.

motor is used it is desirable that the speed should be high enough so that the paddle shaft may be direct-connected with the motor shaft, the speed being regulated by an armature resistance. The consequent loss of power is not objectionable. Experiment showed that if the speed was too high, the emulsifying effect was decreased by an action similar to the "caviation" which caused Parsons so much trouble in his early steam-turbine boat experiments. A speed of 1500 r.p.m., on the average, was found to give the greatest emulsifying effect with the paddle chosen.

Proportion of Water and Oil.—The best results were obtained when the total amount of 60 cc. of liquid was made up of one part of oil to two of water. With smaller amounts of oil, errors

TABLE II.—SAMPLE TEST FOR DEMULSIBILITY.

Clock Time.	Elapsed Time after Stopping of Paddle, min.	Reading of Cylinder at Upper Surface of Emulsion, cc.	Oil Settled Out, cc. (60 minus value in third column).	Rate of Settling, cc. per hour.
9:50	0	60	0	0
9:55	5	57	3	36
10:02	12	47	13	65
10:05	15	43	17	68
10:10	20	41	19	57

due to inaccuracies of measurement became too pronounced, and with greater amounts of oil, the lines of separation between the different layers which formed on letting the emulsion stand, were not distinct. The proportion of 20 cc. of oil and 40 cc. of water also has the advantage of giving the maximum emulsifying effect.

Unit of Measurement.—When it was found that a few oils settled out of the emulsion in one minute or less, it was evident that the resistance of the oil to emulsification could not be measured by the size of globules, viscosity, or other characteristic of the emulsion, unless the method of emulsifying was changed so as to obtain more permanent emulsions. Bryan and Ostwald both observed the amounts of oil and water which settled out of the emulsion after different intervals of time. As

shown by their results, the rate of settling is not constant, and the writer's own tests have confirmed this. Table II for example, shows the results obtained for a certain oil.

The "oil" referred to in the fourth column of Table II is often cloudy, and is to be considered as an emulsion containing a very small amount of water. It was found that the lower meniscus, between the water and the original emulsion, was less distinct, as a rule, than the upper meniscus between the emulsion and the oil, and therefore the upper meniscus was taken as the basis of measurement. It will be noted that the rate of settling, as given in the last column of Table II, is always the average rate between the time of stopping the paddle and the time of observation, and not the rate between two successive times of observation. The maximum rate for the oil considered is 68 cc. per hour, and this is taken as a measure of the resistance of the oil to emulsification.

There is some objection to saying that the emulsibility of such an oil is 68, because a *high* tendency to emulsify would then be represented by a *low* number. But if we use the word demulsibility for the rate of settling, meaning resistance to emulsification, or tendency to de-emulsify, a high value of the rate of settling would then indicate a high resistance to emulsification.

DESCRIPTION OF TEST.

We have now considered all the main features of the test and may codify them as follows:

Twenty cubic centimeters of oil and 40 cc. of water, preferably distilled, are placed in a 100-cc. cylinder, 26 mm. (1 in.) inside diameter, and heated to 55° C. (130° F.). The oil and water are then mixed or emulsified by stirring with a paddle for five minutes at a speed of 1500 r.p.m., and allowed to stand at the same temperature. The paddle is simply a metal plate, 89 by 20 by 1.5 mm. ($3\frac{1}{2}$ by $\frac{13}{16}$ by $\frac{1}{16}$ in.), driven by an electric motor. Observations are taken of the average rate of settling out of the oil from the emulsion, between the time of stopping the paddle and times of observation. The maximum rate of settling, in cubic centimeters per hour, is taken as a measure

of the resistance of the oil to emulsification, and is called the demulsibility.[1]

Supplementary Rules and Precautions.—The following rules and precautions should be noted in making the test:

1. Care should be taken, by shaking the oil can, and otherwise, that the 20 cc. of oil which is taken is a fair sample of the oil to be tested.

2. Before testing, oil should not be kept in a glass bottle exposed to sunlight. (The reason for this rule will be considered later.)

3. Care must be taken that cylinders and paddle are clean from one oil before starting to test another. After wiping the paddle with the finger, to avoid drainage losses, it should be cleaned a second time to avoid contamination of the fresh oil. If soap is used in cleaning a cylinder, the latter should be thoroughly freed from soap by a prodigal use of hot water.

4. It is convenient, to save time, to adopt the rule that no oil shall be allowed to settle for a greater length of time than one hour. While it is possible that there may be some oils which would not reach a maximum rate of settling in one hour, such an oil would be of such a poor quality that it is sufficiently accurate to take its demulsibility as the cubic centimeters of oil which have settled out in one hour.

5. It is convenient, and sufficiently accurate for most purposes, to take readings only on the even minute and to the nearest cubic centimeter. Then the highest possible demulsibility will be obtained if all the oil, 20 cc., settles out in one minute, or at a rate of 1200 cc. per hour. The range of the writer's scale is therefore from zero to 1200. If this rule is adopted, the demulsibility may be very quickly taken from Table III.

Apparatus.—Details of the method of rotating a paddle at 1500 r.p.m. on a vertical axis would not materially affect the result of the test, though it is evident that an irregularity in the speed, caused by driving by hand instead of by electric power, might introduce an error. The first experimental machine was

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[1] As reported to Committee D-2 on Lubricants of the Society, March 5, 1915, the temperature of this test was given as 50° C. (122° F.) and paddle dimensions at 89 by 22 by 1 mm. The word "demulsibility" was adopted after publication of article by Philip. Otherwise the test is given as reported.

made mainly of wood and gave satisfactory results for a period of about nine months, after which the vibration of the paddle became excessive, due to wear of the step bearing and absence of guide bearings. A horizontal shaft motor drove the vertical

TABLE III.—RATE OF SETTLING OR DEMULSIBILITY OF OILS.

EXPRESSED IN CUBIC CENTIMETERS PER HOUR.

Elapsed Time after Stirring, Min.	Reading at Upper Surface of Emulsion, cc.																			
	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69
1	60	120	180	240	300	360	420	480	540	600	660	720	780	840	900	960	1020	1080	1140	1200
2	30	60	90	120	150	180	210	240	270	300	330	360	390	420	450	480	510	540	570	600
3	20	40	60	80	100	120	140	160	180	200	220	240	260	280	300	320	340	360	380	400
4	15	30	45	60	75	90	105	120	135	150	165	180	195	210	225	240	255	270	285	300
5	12	24	36	48	60	72	84	96	108	120	132	144	156	168	180	192	204	216	228	240
6	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200
7	9	17	26	34	43	51	60	69	77	86	94	103	112	120	129	137	146	155	163	172
8	8	15	23	30	38	45	53	60	68	75	83	90	98	105	112	120	128	135	143	150
9	7	13	20	27	33	40	47	53	60	67	73	80	87	93	100	107	113	120	127	134
10	6	12	18	24	30	36	42	48	54	60	66	72	78	84	90	96	102	108	114	120
11	6	11	16	22	27	33	38	44	49	55	60	65	71	76	82	87	93	98	104	109
12	5	10	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90	95	100
13	5	9	14	19	23	28	32	37	42	46	51	55	60	65	69	74	79	83	88	92
14	4	9	13	17	21	25	30	34	39	43	47	51	56	60	64	69	73	77	81	86
15	4	8	12	16	20	24	28	32	36	40	44	48	52	56	60	64	68	72	76	80
16	4	8	11	15	19	23	26	30	34	37	41	45	49	53	56	60	64	68	71	75
17	4	7	11	14	18	21	25	28	32	35	39	42	46	49	53	56	60	64	67	71
18	3	7	10	13	17	20	23	27	30	33	37	40	43	47	50	53	57	60	63	67
19	3	6	10	12	16	19	22	25	28	32	35	38	41	44	47	51	54	57	60	63
20	3	6	9	12	15	18	21	24	27	30	33	36	39	42	45	48	51	54	57	60
21	3	6	9	11	14	17	20	23	26	29	31	34	37	40	43	46	49	51	54	57
22	3	5	8	11	14	16	19	22	25	27	30	33	35	38	41	44	46	49	52	55
23	3	5	8	10	13	16	18	21	24	26	29	31	34	37	39	42	44	47	50	52
24	3	5	8	10	13	15	18	20	23	25	28	30	33	35	38	40	43	45	48	50
25	2	5	7	10	12	14	17	19	22	24	26	29	31	34	36	38	41	43	46	48
26	2	5	7	9	12	14	16	18	21	23	25	28	30	32	35	37	39	42	44	46
27	2	4	7	9	11	13	16	18	20	22	24	27	29	31	33	36	38	40	42	44
28	2	4	6	9	11	13	15	17	19	21	24	26	28	30	32	34	36	39	41	43
29	2	4	6	8	10	12	15	17	19	21	23	25	27	29	31	33	35	37	39	41
30	2	4	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40
35	2	3	5	7	9	10	12	14	15	17	19	21	22	24	26	27	29	31	33	34
40	2	3	5	6	8	9	11	12	14	15	17	18	20	21	23	24	26	27	29	30
45	1	3	4	5	7	8	9	11	12	13	15	16	17	19	20	21	23	24	25	27
50	1	2	4	5	6	7	8	10	11	12	13	14	16	17	18	19	20	22	23	24
55	1	2	3	4	5	7	8	9	10	11	12	13	14	15	16	17	19	20	21	22
60	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20

EXPLANATORY NOTE.—The following examples illustrate the construction of this table: If the reading at upper surface of emulsion is 50 cc. and the elapsed time is 15 minutes, the rate of settling or demusibility $= (60-50)\times\frac{60}{15}=40$ cc. per hour; for a reading of 45 cc. and a time of 10 minutes, demulsibility $=(60-45)\times\frac{60}{10}$ $=90$ cc. per hour.

paddle shaft by means of stepped pulleys, a piece of string serving as a belt. The motor was of $\frac{1}{12}$ h.p. with a normal speed of 2700 r.p.m. The speed was taken with a tachometer applied to the motor shaft, but this was unsatisfactory because it was

found possible to slow down the motor by too hard a push on the tachometer, and there could be no certainty of an absence of slip in the belt.

FIG. 1.—New Apparatus for Demulsibility Test.

In the new instrument, shown in Fig. 1, the resistance used with the first machine was replaced by a sliding rheostat of 340 ohms, to get a more accurate speed control. The vertical shaft, shunt-wound motor, of $\frac{1}{12}$ h.p., has a normal speed of

2550 r.p.m., which may be reduced to 1000 r.p.m., or less, by the resistance. The liquid tachometer, which is shown in Fig. 1 above the motor, was adopted because it has no parts to wear out and runs quietly and continuously. The motor still, the liquid stands at the zero line. The height to which the liquid will rise or fall from the zero line when the motor is started, may be calculated from the formula

$$h=d=\frac{(0.1047n)^2}{4g}R^2$$

where h = height of meniscus above zero line, d = depth of meniscus below zero line, n = r.p.m., R = the inside diameter of the glass tube and g = 980.7 cm. per second^2 (32.17 ft. per second^2).

The formula will give values of h and d in feet, if R is taken in feet and the acceleration of gravity g is taken in feet per second per second. Since the density of the liquid does not appear in the equation, any liquid such as an oil may be used, which does not readily evaporate, and which gives a sharp meniscus. There will be a slight error due to temperature changes if the oil level is not kept adjusted to the zero line.

The scale may be etched on the glass, or may simply consist of a strip of paper, the scale and meniscus both being clearly visible when the motor is running. In graduating the scale it is convenient to find the depression d of the liquid when the motor is running at maximum speed. The speed is given by means of a hand tachometer, inserted in the coupling by removing the paddle shaft. The value of the constant $\frac{R^2}{4g}$ may then be computed from the formula. This avoids the difficulty of measuring the radius of the tube with the required degree of accuracy. The scale is then subdivided by means of the formula.

DEMULSIBILITY OF COMMERCIAL OILS.

Fig. 2 shows a frequency curve for 164 oils tested at the Bureau of Standards, arranged in classes according to the demulsibility. The oils were taken at random as received for test, except that those containing fatty oils are omitted. Table IV shows a classification of the oils according to the use for which they were intended.

The temperature employed was 50° C., or 5° C. below the normal, but the difference in temperature would not mate-

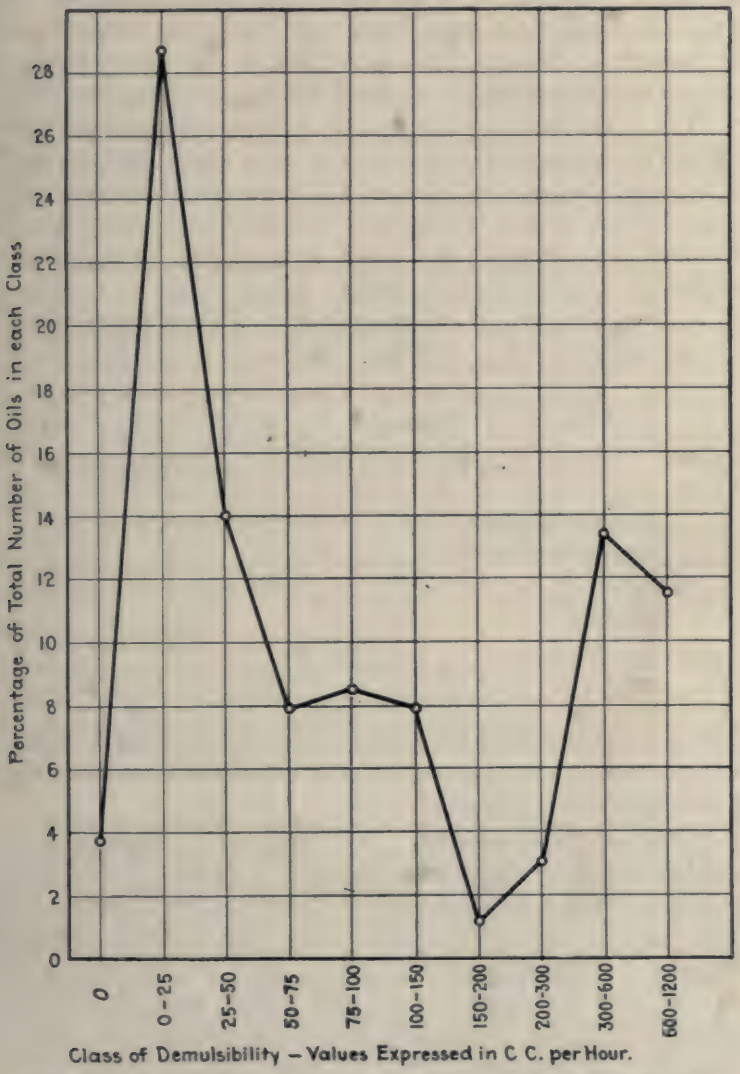

FIG. 2.—Frequency Curve for Demulsibility of 164 Lubricating Oils.

rially change the result. Each point in Fig. 2 indicates the percentage of the total number of oils falling within a certain

class, that is, having a demulsibility between the values selected as limits for the different classes and given at the bottom of the figure. The curve is high near the ends, instead of high in the middle like a normal frequency curve, and indicates that the majority of oils are either very good or very bad. Kerosene and the best transformer oils show a value of 1200.

The writer will not attempt to indicate what values would be proper to place in specifications, or what demulsibilities would be required to give satisfaction. The General Supply Committee, which makes contracts for purchasing supplies for the executive departments and other government establishments, has issued specifications in which a demulsibility of at least 300 is given for turbine and spindle oils. This is approximately the demulsibility of a half-and-half mixture of kerosene and

TABLE IV.—CLASSIFICATION OF 164 TESTED OILS.

Kind of Oil.	Percentage of Each Kind.
Automobile and gas engine	33.5
High-speed engine and turbine	31.7
Machine	15.9
Miscellaneous	18.9
Total	100.0

olive oil. On the other hand, where an emulsifying oil is desired, as in the case of a marine engine or crank-case oil, a demulsibility of zero should be expected.

Cause of Emulsification.—An emulsion is composed of a liquid in which are suspended globules of another liquid, which are coated with a substance which prevents them from uniting. Thus three substances are necessary to form a permanent emulsion, and if nothing is present but a pure oil and pure water, any emulsion which may be formed is very unstable. Greater permanence of the emulsion is caused by the presence of some impurity. As a general rule, a "pale" oil has been filtered more than a "red" oil, and the latter will emulsify the more readily. A certain proof that demulsibility is an indication of purity may be obtained by filtering an oil through bone black

and thus removing certain resinous oxidation products. It has been found possible to greatly increase the demulsibility in this way. Wells[1] says:

"It was found that as a rule dark cylinder oils much more readily emulsified than pale oils."

The Relation between Demulsibility and other Physical Tests of Oils.—Bryan[2] says:

"The specific gravity has no apparent effect on the lubricating quality of an oil, and also no apparent effect on the rapidity with which the oils settle out."

A much more serious consideration is whether viscosity has any effect on demulsibility, and whether the same degree of purity is indicated by a demulsibility of say 300, with one viscosity, as if the same rate of settling were obtained with an oil of another viscosity. The author's results are in conformity with the general experience in showing that, as a rule, oils of high viscosity have a comparatively low demulsibility. One is inclined to believe, however, that this is mainly due to the fact that it is more difficult to purify viscous than fluid oils, though the resistance to the movement of oil and water particles in a viscous fluid has a minor effect. In speaking of cylinder oils, Swarz[3] says:

"Clear mineral oil is in general more easily washed than dark oil containing asphalt, apparently because the asphaltic material easily holds back the soap and is, on its own account, difficult to separate from water. Thus, this may be the explanation why a large majority of the cylinder oils tested here formed emulsions with water at 85° C."

Relation between Demulsibility and Chemical Tests.—There are already so many tests for lubricants, now or formerly in use, that it would seem undesirable to add to the list unless it could be shown (1) that the new test is a simpler, shorter or easier method of accomplishing the same result as tests already in use, or (2) that information can be derived which can not be attained by other means. Philip gives two tables of experimental results, which show that he could detect smaller amounts of the emulsifying ingredient by means of his stirring test than

[1] *The Engineer*, Aug. 14, 1903, p. 155.
[2] *Journal*, Am. Soc. Naval Engrs., Vol. 26, p. 557 (1914).
[3] "Mitteilungen aus dem königlichen Material Prüfungsamt," 1909, p. 22.

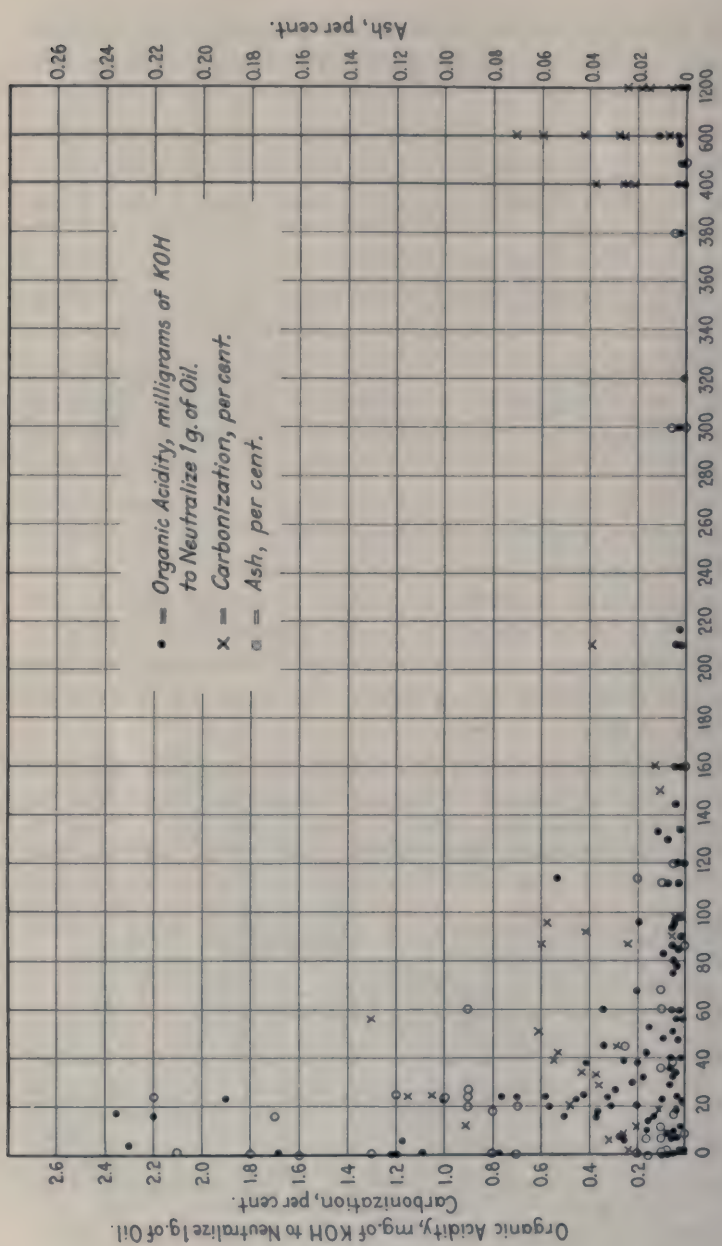
Organic Acidity, mg. of KOH to Neutralize 1 g. of Oil.
Carbonization, per cent.
Ash, per cent.
• = Organic Acidity, milligrams of KOH to Neutralize 1 g. of Oil.
× = Carbonization, per cent.
○ = Ash, per cent.
Demulsibility.

FIG. 3.—Comparison of Demulsibility and Chemical Tests.

by means of chemical tests, and this may explain why it has been the common experience of chemists that of two oils which appeared to be identical, one would emulsify in use and the other would not. Philip says:

"As far as the use of a mineral lubricating oil for turbines is concerned, the determination of the demulsification value is of greater use than any other test which can be made. . . . Out of between 700 and 800 samples of such oils which have been tested and passed as satisfactory as possessing demulsification values of 90 per cent or over, not a single one has given any trouble whatever due to the formation of emulsions under the practical conditions of use on a forced lubricating system on steam turbines."

Unfortunately Philip has neglected to give a sufficiently detailed description of his apparatus to make its duplication possible, so that the writer is not in a position to derive a conversion factor between the "demulsification value" of Philip and "demulsibility." Attention might be called, however, to the fact that, during the past year, in which the government departments in the District of Columbia have been using a turbine oil, selected mainly on account of its high demulsibility, there has not been a single complaint in regard to emulsification.

A comparison between demulsibility and chemical tests of the Bureau of Standards confirms the greater sensitiveness of a stirring test. In Fig. 3 organic acidity, percentage of carbonization and percentage of ash are plotted against demulsibility. Acidity is expressed as the fraction of a milligram of potassium hydroxide (KOH) required to neutralize one gram of the oil, and the carbonization is found by Waters'[1] method. It will be seen that the correlation between carbonization and demulsibility is not very marked, but that the demulsibility is low whenever the acidity or percentage of ash is high. A low value in the chemical tests, however, is not always accompanied by a high demulsibility. Waters[2] has shown that organic acidity is increased by exposure to sunlight, and as shown be Fig. 3, the demulsibility decreases as the acidity increases. Thsi is the reason for the rule offered by the writer that samplys of oil intended for demulsibility tests should not be left in the sunlight in glass bottles.

[1] *Reprint No. 160*, Bureau of Standards.

[2] *Reprint No. 153; Technologic Paper No. 4, Technologic Paper No. 73*, p. 9, Bureau o Standards.

DISCUSSION.

Mr. Conradson.

MR. P. H. CONRADSON.—Mr. Herschel's paper certainly shows that he has gone into the subject in a most thorough and scientific manner. The paper as it now stands reviews the work that has been done by other investigators on the subject in a very satisfactory manner, while the apparatus and test method described form a valuable contribution to this field of the Society's activities, and are entitled to careful consideration.

Considering the apparatus and test method and the calcula-

TABLE I.—STEAM TURBINE LUBRICATION: COMPARATIVE TESTS OF TWO OILS, ONE YEAR IN SERVICE.

TWO 1000 K. W. VERTICAL CURTIS TURBINES, CIRCULATING OIL SYSTEM, 18 BBL. OIL ADDED FOR EACH TEST, AVERAGE 19 TURBINE HOURS DAILY FOR EACH TURBINE.

Properties.	Oil No. 1.		Oil No. 2.	
	Original.	After 1 yr. Service.	Original.	After 1 yr. Service.
Flashing point	395° F.	395° F.	400° F.	400° F.
Burning point	450° F.	455° F.	455° F.	460° F.
Gravity at 60° F	33.0° B.	32.5° B.	29.7° B.	28.9° B.
Color	Light yellow	Red	Red	Dark red
Odor	Good	Pungent	Good	Pungent
Acidity calculated as oleic acid	0.02 per cent	4.12 per cent	0.02 per cent	0.60 per cent
Cold test	Zero° F.	Zero° F.	+27° F.	+22° F.
Heat test 425° F—6 hr	Good	Poor	Good	Poor
Emulsifying test	Good	Good	Good to fair	Poor
Viscosity univ., 100° F	130	170	229	267
Maumené test	5° F. rise	25° F. rise	4° F. rise	9° F. rise
Saponification number Mg.KOH per gram of oil	0.03	2.63	0.06	0.38
Saponifiable matter, using factor 195	0.17 per cent	13.49 per cent	0.34 per cent	1.95 per cent

During the year oil No. 1 was tested, it was necessary to clean out the circulating oil system once on account of the large accumulation of muck formed. The oil system with oil No. 2 was not cleaned out during the tests, but at the expiration some muck had formed in different parts of the system. Oil No. 1 formed a light voluminous muck, which upon heating to 150° F. melted down to a black tough mass. The muck from oil No. 2 was much denser and darker, to begin with.

tion of results as set forth in the paper, from a technical-practical standpoint, such as practical users of lubricants must do, they might be considered rather complicated in laboratories where tests of lubricating oils are not made a specialty. From the standpoint merely of demulsibility, without considering other properties of the oil, the method and calculations—although the

latter are somewhat complicated to my mind—I consider capable of giving good results, especially with some modifications, although I have not had actual practical experience with the apparatus myself. Mr. Conradson.

However, from a practical service standpoint in connection with steam turbine lubrication, it seems to me that Mr. Herschel should take into account not only the demulsibility, but the emulsibility of the oil, the condition of the separated water, the condition of the emulsified layer, if any, and the condition of the separated oil above the emulsified layer, at the end of the test. These details, which can be easily worked out and standardized, would, I think, increase the value of his method.

TABLE II.—TESTS OF TURBINE OIL.

SHOWS GRADUAL INCREASE OF ACIDITY, SAPONIFIABLE MATTER AND RISE IN TEMPERATURE OF MAUMENÉ TEST OF SAMPLES TAKEN FROM TIME TO TIME FROM THE CIRCULATING SYSTEM OF STEAM TURBINES.

Turbine Oil.	Acidity.		Saponification Number, Mg. KOH per gram of oil.	Saponifiable Matter, using factor 195, per cent.	Maumené Test; Rise in deg. Fahr.
	Acid Val. Mg. KOH per gram of oil.	Calculated as Oleic Acid, per cent.			
Original Oil A	Trace	0.02	0.03	0.17	5
Oil A from Service B	0.0021	1.05	0.68	3.52	12
C	0.0042	2.18	1.45	7.47	18
D	0.0092	4.50	2.63	13.49	25
E	0.0151	7.35	4.18	21.40	29
F	0.0197	9.53	....	....	36

Mr. Herschel states in his introductory remarks that: "In order to reduce the danger of the oil becoming unserviceable on account of chemical changes, it has been found advisable to use only pure mineral oils, free from acid or paraffin, but the idea, once prevalent, that oils 'wear out' in a mechanical sense has been generally abandoned." Further, quoting from Thurston: "A mineral oil is usually just as good after use as before, apart from the impurities, which are removed in filtering." I do not know exactly what the author of the paper means by the statement that "oils wear out in a mechanical sense." It has been my experience, after following up the behavior of pure mineral oils in steam turbine service for many years, that the

Mr. Conradson. oils certainly do break up, and that they are not usually as good after being used as before, apart from impurities which can be removed in filtering.

I present herewith two tables taken from laboratory notes of investigations of the behavior of turbine oils in actual service which I made some years ago. I believe these will be of interest in showing how even the best grade of mineral oils used in turbine lubrication may change.

I should like particularly to call attention to the changes that take place in the formation of oxidation or acid products; to the increased formation of saponifiable matter that is produced by the breaking up of the oils; to the iodine numbers of the oils before and after they have been used in service for a year or more; and to the enormous increase in the rise of temperature in the Maumené test, as well as the formation in large amounts of solid materials thrown out from the oils, clogging up the circulating system and filters. I may state further that the formation of solid oxidation and polymerization products thrown out from an oil during service may occur as well with oils that originally do not emulsify or are perfect in the quality of demulsification.

Mr. Stalknecht. MR. A. C. STALKNECHT.—It would be interesting to have Mr. Conradson explain why this phenomenon occurs. Going back to the oil distiller: I have known this trouble to occur in crank cases and it could not be accounted for in service, so responsibility could only be traced back to the maker of the oil. Investigation determined that although the same quantity of the same crude oil had been used and practically the same quantities of the identical distillates had been driven off, we still got the same liver-like or saponified agglutinations. Our investigations led us to an examination of the steam boilers and there we found the flues covered thickly in some places with a coating of sulfate-of-lime scale. We had previously determined in the distillations that our temperatures were high enough, so we then decided that it must be a question of the quantity of steam. By thoroughly removing all scale from the steam boiler we obtained sufficient steam to form the necessary partition between the metal and the oil to prevent contact of the two. Carbonization and the following thickening or saponification of parts of

the crank-case oil were obviated. We accordingly decided that the trouble could be avoided by using sufficient steam. I don't know of a higher authority on this question than Mr. Conradson, and hope that he may give us some further light on that subject. Mr. Stalknecht.

MR. CONRADSON.—I am not a refiner, but there are three or four expert refiners here who might answer your question. Mr. Conradson.

MR. WINSLOW H. HERSCHEL.—I should like to reply to Mr. Conradson's remarks. I do not think there is any difference of opinion whatever between him and myself in regard to the deterioration of an oil in use; I simply think the idea has changed from that of a purely mechanical wearing out to one of a chemical break-down, polymerization or some other process not clearly understood. It is undoubtedly true that, given a sufficient length of time, an oil will deteriorate and increase in acidity, some oils much more rapidly than others; and as I have shown in my paper, as a general rule the oils high in acidity have a low demulsibility. There is a very close connection between acidity and demulsibility. Mr. Herschel.

Mr. Conradson thinks I should observe the condition of the emulsified layer. I have not emphasized this point because, in the turbine oils in which I am most interested, there is no emulsified layer to observe. The entire emulsion settles out in less than five minutes in any oil which we should consider for purchase, and therefore we cannot observe the emulsified layer. As a matter of fact, in other oils where there is such a layer, I do observe whether the water and the oil that settle out are cloudy. But I think it is an advantage of my test that I do not have to observe these conditions of cloudiness in the case of turbine oils which are suitable for purchase, because an exact amount of cloudiness can hardly be defined in specifications or enforced in a contract. Therefore, the only thing to be defined and enforced is the value of the demulsibility, and I believe that if that value is sufficiently high, the oil will be found satisfactory as regards emulsification.

Mr. Conradson speaks of the method being complicated. I think the only thing that could be so regarded is the method of finding the value of demulsibility from the observed readings, and that does not appear to me to be very difficult if Table III, given in the paper, is used. On the other hand, the test has the

Mr. Herschel. great advantage of speed, because in those oils in which we are most interested, it takes only five minutes to stir and five minutes to settle out. One does not have to wait any definite length of time to complete the test; so that the better the oil, the sooner the results are obtained. If one simply wishes to know whether an oil comes up to the required specifications, the test may be completed in ten minutes.

Mr. Conradson. MR. CONRADSON (*by letter*).—Mr. Herschel, in his reply to my remarks on his paper, says: "As I have shown in my paper, as a general rule the oils high in acidity have a low demulsibility. There is a very close connection between acidity and demulsibility."

I cannot agree to these statements. I have had occasion to examine many hundreds of samples of turbine oils before they were put into service and to follow up their subsequent behavior in continuous service all the way up to three or four years, or longer; the fresh oils were free from acidity to begin with, and after various lengths of time and service conditions, developed from 0.50 per cent up to 3 or 4 per cent acidity, calculated in terms of oleic acid; and I have so far found, with very few exceptions, that turbine oils having a high degree of demulsibility, that is, free from emulsion forming materials, have not developed low demulsibility during service, even though the percentage of acidity has become high. In fact, my experience has been in general that turbine oils containing when fresh more or less emulsion-forming materials, after being in service for some time, with the usual increase in acidity, gain a higher degree of demulsibility. At times oils removed from service contain in suspension more or less foreign materials that may cause emulsion or low demulsibility; also, at times, they become contaminated with fatty or other oils; but if these materials are removed the oils as such, as a general rule, increase in demulsibility during service, although the acidity may be largely increased.

I speak from many years of exceptionally good opportunities to follow up turbine oils in actual service in hundreds of power houses all over the country.

Mr. Herschel seems to lay great stress in the advantage of his test method on account of the short time required, five minutes stirring and five minutes to settle out. Further he

states: "The entire emulsion settles out in less than five minutes in any oil which we should consider for purchase." Mr. Conradson.

It is unfortunate that Mr. Herschel did not furnish a table of tests, such as viscosity, specific gravity, congealing point, etc., of the oils he tested, and from which he has drawn conclusions in his demulsibility investigations. My experience in testing hundreds of samples of turbine oils from various sources is, that it takes a much longer time for the general run of such oils to settle out—whether by his method or some other—if the temperature ranges as given in his method are used. I admit that some turbine oils very low in viscosity and with low congealing points, may settle out in a comparatively short time; but most of the turbine oils used, and with good success, in the larger steam turbines having a capacity from 5000 to 15,000 kw. and requiring oils with relatively high viscosity, say from 250 to 400 at 100° F. (Saybolt Universal viscosimeter), generally at the best take from 30 to 60 minutes to settle out, and such oils have given good results in prolonged service, even under exacting conditions.

Mr. Herschel's test method and apparatus, as stated in the beginning of my discussion, are all right, but the conclusions he draws from his investigations may be questioned, as above indicated. We have to deal with conditions as found in actual varied service, as well as materials that are available in the market for such services, and which have proven satisfactory, and if we draw up specifications for turbine oils in accordance with views expressed by Mr. Herschel, namely, requiring a turbine oil to settle out free from emulsion inside of five minutes, we would find difficulty in securing such oils, to say nothing of the increased price due to such requirements.

MR. HERSCHEL (*Author's closure by letter*).—It is admitted by Mr. Conradson in his last sentence, and generally known, that oils emulsify more readily after use.[1] If the contamination is removed by filtration, the oil may become more thoroughly purified than when it was new, and consequently emulsify less readily.[2] Mr. Herschel.

The length of time it takes for any particular oil to settle

[1] See Archbutt & Deeley, "Lubrication and Lubricants," p. 539.
[2] See *Technologic Paper No. 73*, Bureau of Standards, p. 9.

Mr. Herschel. out from an emulsion depends upon the method by which the emulsion is made, and therefore Mr. Conradson's figures in regard to the time of settling, obtained by other methods, do not apply to the present case. The 164 oils referred to in Table IV were taken at random and contain oils of such viscosity, specific gravity and congealing point as are actually used for the purposes stated. No difficulty has been experienced in procuring oils, at a reasonable price, which will settle out in five minutes or less, when emulsified by the method recommended.

EMULSIFICATION OF MINERAL LUBRICATING OILS: APPARATUS AND TEST METHOD.

By P. H. Conradson.

SUMMARY.

The paper describes a simple apparatus and test method to determine the emulsibility and demulsification (demulsification value) of lubricating oils such as steam-turbine, steam-engine, and crank-case oils, which have been found to give satisfactory results in the writer's laboratory considered from a practical service standpoint.

The method is briefly as follows: Twenty cubic centimeters of distilled water and 100 cc. of the oil to be tested are placed in a suitable cylinder, which is described in the paper. The mixture is churned by passing steam through it at ordinary pressure. The cylinder containing the mixture is then immersed for one hour in a water bath kept at a temperature of 130° F., removed from the bath, and the contents are examined for:

1. The number of cubic centimeters of separated clear or turbid water;

2. The number of cubic centimeters of separated emulsified layer;

3. The number of cubic centimeters of separated clear or turbid oil above the emulsified layer; and

4. The percentage of water or moisture in the separated oil above the emulsified layer.

The number of cubic centimeters of clear or turbid oil above the emulsified layer, less the percentage of water or moisture contained in the oil, is the percentage of demulsibility of the oil.

The method described gives in majority of cases both the demulsification value and the emulsion-forming property of the oil, besides other valuable data in connection with so-called emulsifying tests.

EMULSIFICATION OF MINERAL LUBRICATING OILS: APPARATUS AND TEST METHOD.

By P. H. Conradson.

The simple apparatus and method of testing described, to determine the emulsibility and demulsification (demulsification value) of lubricating oils such as steam-turbine, steam-engine, and crank-case oils, have been used in daily routine work for the last year or more in the writer's laboratory, and have been found to give satisfactory results considered from a practical service standpoint. They are simplifications of a much larger apparatus and more laborious method used previously for several years by the writer, in connection with investigations of principally turbine oils in actual service, with reference to trouble from emulsifications.

Others who have suggested apparatus and test methods in connection with emulsion tests of lubricating oils, as Phillips,[1] Herschel[2] and Bryan[3] have written at considerable length and in elaborate detail on the subject; therefore, this paper will be confined to a brief description of the apparatus, method of testing, and interpretation of results.

Apparatus.

Fig. 1 needs but little explanation. The apparatus consists of a 4-pint copper retort, provided with a delivery tube, which is joined to a metal or glass pipe having an inside diameter of about $\frac{5}{16}$ in. and about 15 in. long from the elbow. The lower end of this pipe is cut off diagonally to prevent thumping.

The glass cylinders are graduated to 250 cc. They have an inside diameter of about $1\frac{7}{16}$ in. and a length of about $9\frac{1}{2}$ in. from the bottom to the 250-cc. mark. They are $11\frac{1}{2}$ to 12 in. in overall length, and are made of thin glass, with a flat bottom.

[1] *Journal*, Soc. Chem. Ind., July 15, 1915.
[2] Bureau of Standards, Washington, D. C.
[3] Experimental Station, Navy Department, Annapolis, Md.

In place of a copper retort for the generation of steam, a glass flask or any other suitable source of steam supply may be used; likewise, ordinary 250-cc. graduated glass cylinders, of dimensions given above, may be used where emulsion tests are required only occasionally.

METHOD OF TESTING.

The cylinder is filled with distilled water up to the 20-cc. mark, then 100 cc. of the oil to be tested are added. To churn

FIG. 1.—Oil-Emulsification Testing Apparatus.

the mixture, steam at ordinary pressure is conducted through this oil-water mixture for ten minutes. The amount of steam passed through is regulated in such a way so as to prevent the mixture from splashing over the top of the cylinder, but the rate may be as rapid as is practical. This is easily regulated by the height of the gas flame.

The churning is begun from the time the temperature of the mixture has reached 200° F., or when the steam as such

passes off the mixture. It usually takes from 1 to 1½ minutes to reach this temperature, depending somewhat on the body or viscosity of the oil. However, even churning with steam for 15 minutes does not seem to make any difference in the results.

When the churning is completed, the cylinder is immersed for one hour in a water bath, kept at a temperature of 130° F. During this time the cylinder and its contents are momentarily inspected at intervals to note the behavior of the oil mixture. At the expiration of one hour the cylinder is removed from the water bath and the contents are examined for the following:

1. The number of cubic centimeters of separated clear or turbid water;

2. The number of cubic centimeters of separated emulsified layer;

3. The number of cubic centimeters of separated clear or turbid oil above the emulsified layer; and

4. The percentage of water or moisture in the separated oil above the emulsified layer.

The number of cubic centimeters and condition of the emulsified layer is an indication of the emulsion-forming property or quality of the oil.

The number of cubic centimeters of clear or turbid oil above the emulsified layer, less the percentage of water or moisture contained in the oil, is the percentage of demulsibility of the oil.

The condition of the separated water or watery liquid under the emulsified layer, if any, gives an indication also of the behavior of the oil in actual service.

The amount of water held in the oil above the emulsified layer may be determined as follows:

The oil above the emulsified layer after the expiration of the test is carefully drawn off and shaken; then 20 cc. are mixed with 80 cc. of 88° Baumé gasoline (from Pennsylvania Crude) in a graduated, flat-glass precipitating tube having the lower end drawn out. The oil-gasoline mixture is kept at a temperature not over 80° F. for one hour, or the water or watery liquid may be separated from the oil-gasoline mixture by means of a centrifuge. The amount of water or watery liquid is read off and calculated to percentage by volume and subtracted from

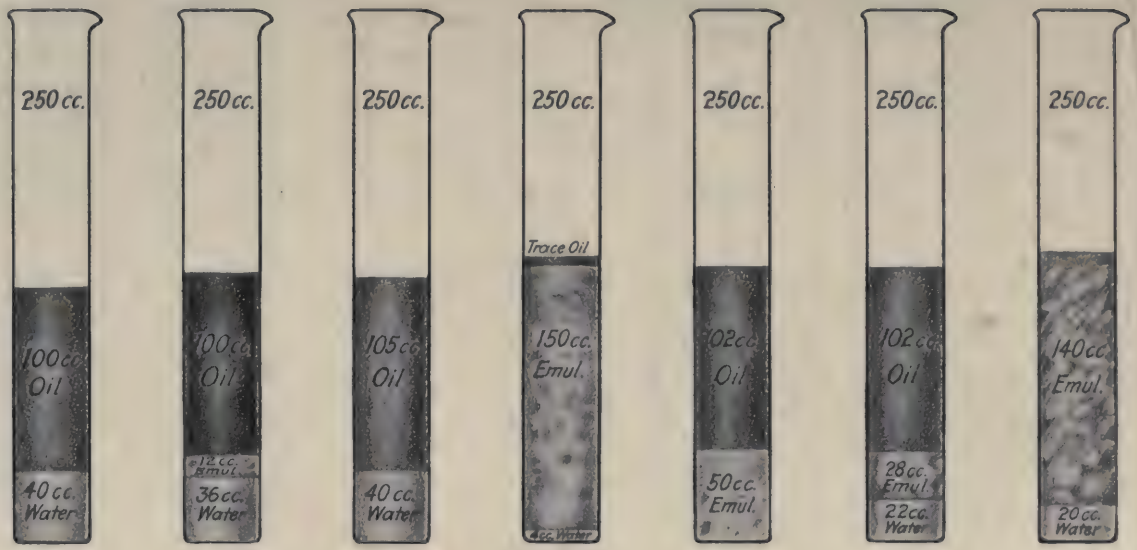

FIG. 2.—Representative Lubricating Oils at Expiration of Emulsification Tests.

the oil above the emulsified layer. Of course, this determination is only necessary when the oil above the emulsified layer appears to contain an appreciable amount of water.

INTERPRETATION OF RESULTS.

Fig. 2 illustrates the behavior of seven representative oils with this method, as they appear after expiration of the tests.

Table I gives detailed results of tests of these oils. The first turbine oil shows the oil entirely free from emulsifying property or elements, only retaining a very small percentage of

TABLE I.—EMULSIFICATION TESTS OF LUBRICATING OILS.

Oil No.	Kind of Oil.	Separated Water, cc.	Condition of Water.	Emulsified Layer, cc.	Kind of Emulsion.	Separated Oil, cc.	Condition of Oil.	Moisture in Oil, per cent.	Demulsibility, per cent.
1	Turbine.....	40	Clear	None	....	100	Slightly turbid	0.2	99.8
2	Turbine.....	36	Slightly turbid	12	Light foamy	100	Turbid	1.0	99.0
3	Crank Case.	40	Clear	None	....	105	Very turbid	5.0	95.0
4	Crank Case.	4	Clear	150	Heavy thick	Trace	Practically all emulsion	...	0.0
5	Engine.....	None	....	50	Thick milky	102	Very turbid	4.0	96.0
6	Engine.....	22	Clear	28	Light foamy	102	Turbid	3.0	97.0
7	Spindle.....	20	Milky	140	Thick milky	None	All emulsion	...	0 0

moisture or water after the expiration of the test. The second turbine oil shows quite a little of emulsified layer, but the condition of the emulsified layer is light and foamy, not compact or creamy. The amount of water or moisture retained in the oil is much higher than in the first oil.

Consider next the two samples of crank-case oil: The first oil shows ready separation of water, which is clear and has no emulsified layer, but the oil after the test retains about 5 per cent of water. With the other crank-case oil only a very few cubic centimeters of water are separated at the expiration of the test, and a very large amount of emulsified layer of a

heavy thick nature is shown; in fact the whole mixture is a heavy emulsion without separation.

The first sample of engine oil shows at the end of the test a thick milky emulsion with practically no separation of water, and the separated oil above the emulsified layer contains about 4 per cent of water. The second sample of engine oil shows considerable amount of separation of water and much smaller amount of emulsified layer; this layer is of a light foamy nature.

It should be particularly noticed in these two cases that while 100 cc. of oil were used in the tests, 102 cc. of separated turbid oil were found; deducting the amount of moisture or water found in the separated oil, 4 and 3 per cent, respectively, gives 96 and 97 per cent of demulsibility. This is a clear illustration of the importance of giving a complete statement in the report of the behavior of the oil or oils in the emulsifying test, as simply stating the percentage of demulsibility is clearly insufficient, and in cases of this kind would be seriously misleading.

DISCUSSION.

Mr. Herschel. MR. WINSLOW H. HERSCHEL.—I think that perhaps there is some misunderstanding as to whether these turbine oils really emulsify or whether they do not emulsify. If it is not a "creamy" emulsion, it is not a true emulsion at all; and therefore oils which settle out within five minutes, indicating that they are satisfactory, I believe may properly be said not to have emulsified at all.

Mr. Mackenzie. MR. K. G. MACKENZIE.—In the investigation of oils for turbine or forced-feed lubrication we have two properties to consider:

1. The tendency of the oil to emulsify with water, forming a more or less stable emulsion;

2. The tendency of the oil to decompose in service, as, for instance, by oxidation or polymerization.

The test described by Mr. Herschel[1] gives us a method which will throw light on the tendency of a given oil to emulsify. I sincerely trust, however, that the work on this very important class of lubricants will not stop at this point, but that a test may be devised by which the resistance of the oil to decomposition in service can also be determined or at least indicated. Unfortunately an oil, to use Mr. Herschel's nomenclature, may have high demulsibility and yet decompose very quickly in service. By a combination of tests which will indicate both the tendency of the oil to emulsify and to decompose, we shall have made great progress in the investigation of this class of oils.

Mr. Herschel. MR. HERSCHEL.—I thoroughly agree with Mr. Mackenzie as to the desirability of having a test to determine the resistance of an oil to polymerization, and I am sure the Society would be greatly indebted to any one who would devise and describe such a test. I think, nevertheless, that it is taking one important step if we can make sure that a given oil will not emulsify within 24 hours, and without the emulsification test we could not be

[1] "Quantitative Test for Resistance of Lubricating Oils to Emulsification," p. 248.—ED.

sure even of that. I do not claim that an oil which will pass the emulsification test is sure to last indefinitely in a turbine without polymerization. **Mr. Herschel.**

MR. P. H. CONRADSON.—Mr. Mackenzie's remarks are very well taken, and I think the problem should be considered by Committee D-2 on Lubricants at the proper time. **Mr. Conradson.**

Mr. Herschel speaks about specifications. I think that the Bureau of Standards, as well as the various Government departments, have gone further in this matter of specifications than other consumers of lubricants. I recently noticed that the specifications issued by the General Supply Committee of the Government buildings in Washington, a committee which is buying considerable quantities of lubricating oil, specify the demulsification test. The Navy Department also specifies such a test. The development of proper test methods is, however, an entirely different thing from the writing of specifications, although the two are closely related to one another; and it is to be regretted that while we are developing the test methods, we are not in a position to consider, at least to a certain extent, the question of specifications adaptable to the various lubricants.

AN APPARATUS FOR TESTING THE STANDARD CAST-IRON ARBITRATION BAR.

By H. L. Morse.

SUMMARY.

The paper presents in some detail the design of an apparatus for testing the standard cast-iron Arbitration Bar.

The deflection-measuring device of the apparatus utilizes the Ames dial by means of a thrust spindle and bell-crank, in such a way as to give an accurate measurement of deflection at the instant of fracture of the bar, and at the same time avoid any probability of injury to the dial.

AN APPARATUS FOR TESTING THE STANDARD CAST-IRON ARBITRATION BAR.

By H. L. Morse.

The apparatus herein described was designed by the writer for use on the 100,000-lb. Emery testing machine in the Ordnance Laboratory at Watertown Arsenal and was constructed in the Arsenal shops. The investigation of the comparative merits of the Arbitration Bar, a standard of this Society, and the tension test (Ordnance standard), as criteria of cast iron for ordnance purposes, which required this testing apparatus, will unfortunately not be completed for publication for some months. As there has appeared to be a considerable local demand for and interest in such an apparatus, the writer has ventured to submit it to the Society by itself as a testing device, designed for and accomplishing its special end in a simple, accurate and satisfactory way; cheap to construct, rugged in design, and applicable with but slight modification to any type of testing machine.

Requirements of the Apparatus.

The problem presented was to measure, at any load or at rupture, the deflection at the center of a practically cylindrical rough casting of a diameter of $1\frac{1}{4}$ in. supported at both ends, with a free span of 12 in. (variable to 16 or 18 in.) and stressed transversely by a load applied at the center of its span. Variability of length between supporting points was suggested for the reason that the best length of the arbitration bar might ultimately be determined to be greater than 12 in., and the suggested alternative lengths, 16 and 18 in., were therefore to be provided for in the design.

The machine to which the apparatus was to be applied is the familiar Emery horizontal hydraulic type, shown in Fig. 1. At the left is the loading cylinder, adapted to be pin-locked to its two supporting rods at various distances from the weighing cylinder for different lengths of specimen, and to which oil from

an accumulator, located across the room, is supplied under pressure by the system of piping seen rising at the left from the floor and appearing above as an inverted V. At the right is the weighing cylinder connected by piping (not shown) to the hydraulic scale enclosed in the cabinet just beyond it.

The transverse stressing to destruction of a specimen of the

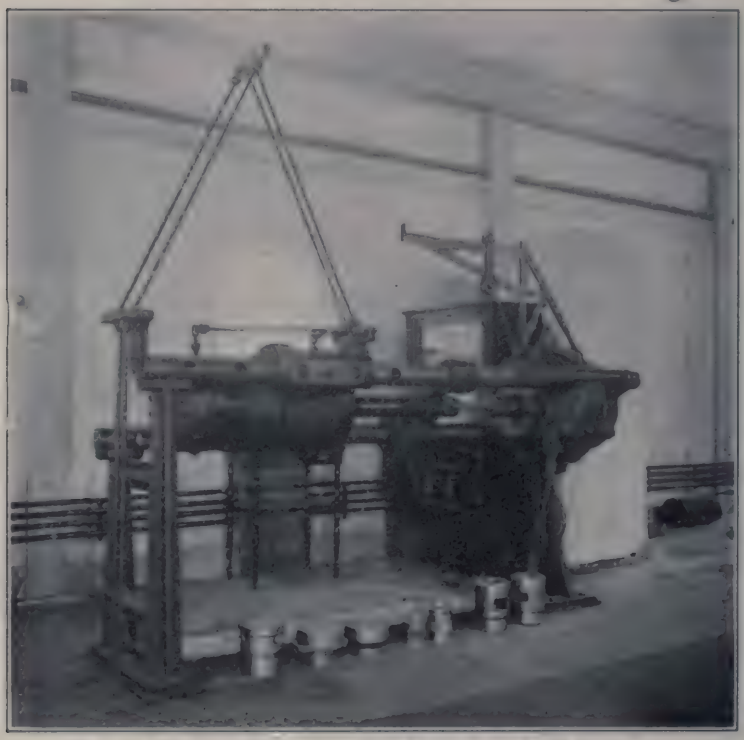

FIG. 1.—Emery Horizontal Hydraulic Testing Machine.

dimensions of the arbitration bar within the standard limits, 20 to 40 seconds, is easily accomplished on this machine by means of a very simple adjustment of the loading control valves regulating inflow and outflow.

As the starting point of design was conditioned by the design of the machine just described, to the two pistons of which the loading and weighing elements of the apparatvs must be attached,

so the final, as well as the intermediate character of the train of elements of the deflection-measuring device was conditioned by the choice of the Ames dial as the deflection indicator. This dial was chosen because several were available, the laboratory personnel was accustomed to its use, and simplicity, durability and sufficient accuracy seemed therein to be admirably combined.

Other conditions to be met by the design were as follows:

1. No part of the deflectometer should be under appreciable stress;

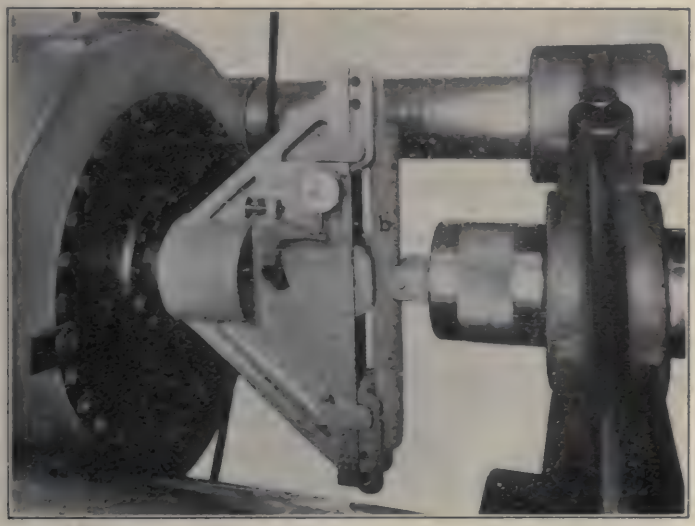

FIG. 2.—Apparatus in Place on the Testing Machine.

2. Rupture of the specimen should not occasion inordinate shock on any frail element, especially on the Ames dial;

3. Quick and accurate zero adjustment, independent of scale adjustment of the dial, was required for easy and rapid work and the use of the full range of the dial, in case the test bars should be slightly warped or thickened at the points of support.

THE DESIGN.

Fig. 2 shows the apparatus in the testing machine, with the arbitration bar in place ready for test. This view is taken

from the opposite side of the testing machine to that shown in Fig. 1. The principal plane of the apparatus is vertical or nearly so.

Fig. 3 shows the principal parts of the apparatus, and Fig. 4 shows the deflection-measuring device still further separated.

Fig. 5 shows in diagrammatic form the relation and interaction of the principal elements as assembled and operating.

To avoid indefiniteness in further description, there is introduced here a list of all parts of the apparatus in the terms to be used, and opposite each item of the list the letter, if any, by which it is indicated in Figs. 2, 3, 4 and 5.

Reference Letter.	Number of Pieces.	Name of Part.
b		Arbitration bar.
a	1	Loading head.
p	1	Loading head pin.
c	1	Weighing head.
q	2	Weighing head pin.
k		Key-way in weighing head into which fits the deflectometer key (not shown).
i	1	Spindle.
	1	Spindle pin.
d	1	Deflectometer.
		Parts of Deflectometer.
e	1	Plate.
	1	Key.
f	2	Clip.
h	2	Clip spring.
	2	Clip spring screw.
l	1	Lever.
r	1	Lever spring.
m	1	Slide.
n	2	Slide guide.
	6	Slide guide screw.
g	1	Slide gib.
	2	Slide gib screw.
s	1	Adjusting screw.
o	1	Dial.
t	1	Dial attaching screw.

The load is applied and the bar is supported by hardened tool steel pins *p* and *qq*. These are ground to easy drive fit (for replacement or change of span) in holes bored in the steel castings *a* and *c*, which have hollow hubs threaded to fit the piston rods of the testing machine. These pieces constitute the entire loading and weighing device.

The deflectometer *d* is held against the face of the casting *c*, and against the under elements of the supporting pins *qq* by the clips *ff* and stiff clip springs *hh*. The pins *qq* are an easy drive

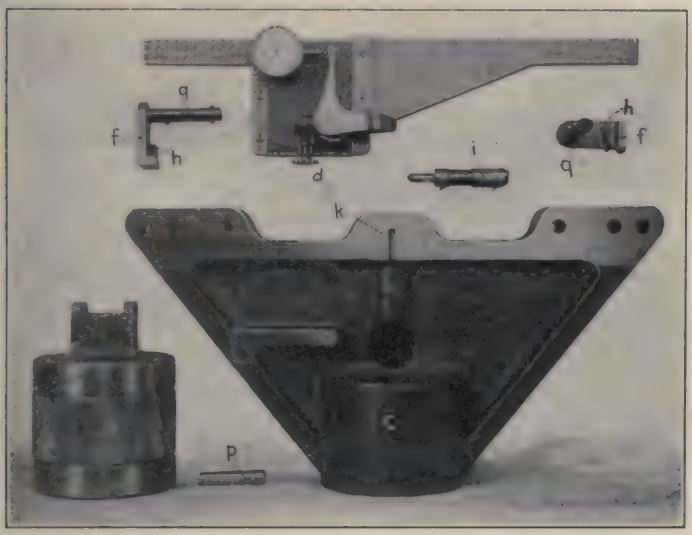

FIG. 3.—Apparatus Separated into Its Principal Parts.

fit in clips *ff*. In the center of the back of the deflectometer plate *e* is fitted a key (not shown), which fits with an accurate slide fit in the key-way *k*, thus accurately centering the deflectometer but permitting free relative movement along the direction of the loading force, and hence avoiding any strain of the the deflectometer plate.

On the face of the deflectometer plate are screwed two guides *nn* for the slide *m*, the inner one of which carries the gib *g*, adjustable by two gib screws (not shown). The movement of the slide *m* is in a line accurately perpendicular to the edge of the deflectometer plate *e*, which bears against the pins *qq*, and accurately

parallel to the principal plane of the casting *c*. The distance parallel to the plate *e*, from the outer guide *n* to the plane perpendicular to plate *e* containing the principal axis of the casting *c*, must be very accurately finished, so that the distance between the spindle *i*, which is located in that axis, and a plane through the pintle axis of the lever *l*, parallel to the motion of the slide, will be the same as the distance between the axis of the spindle of dial *o* and a plane perpendicular to the motion of the slide *m*, containing the pintle axis of the lever *l*. Accuracy in the particulars just noted insures accurate conversion of the deflection

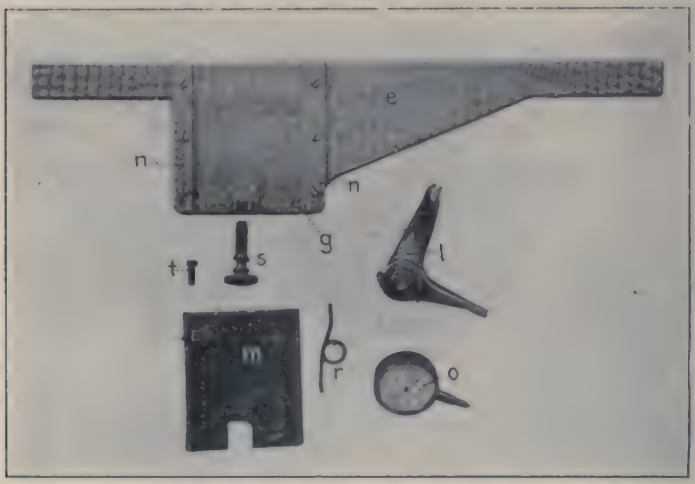

FIG. 4.—Parts of Deflectometer.

movement of the center of the bar through spindle *i* and lever *l* to the spindle of the dial *o*.

The bearing for the pintle of lever *l* is bored in a lug cast on the slide *m*, accurately perpendicular to the under face of that slide and at accurate distances from the dial spindle and the left edge (upper edge in Fig. 2) of the plate, for reasons indicated in the preceding paragraph. The active faces of the ends of this lever are case-hardened and ground to lie in two perpendicular planes through the axis of its pintle. One arm of the lever is bent to recover the offset from the spindle of the dial *o* to the principal plane of the casting *c*, in which the axis of the spindle *i*

is located. A stop on the lever and a corresponding stop on the pintle hub prevent counterclockwise motion of the lever beyond the angle at which the plane through the dial end is

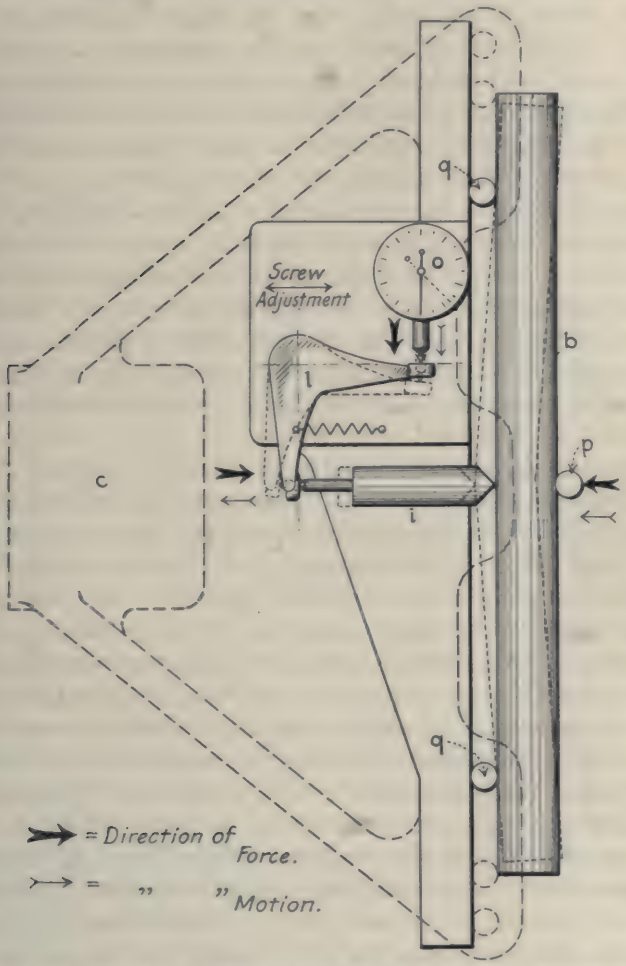

FIG. 5.—Diagram of Apparatus showing Relation and Interaction of Principal Elements.

parallel to the slide guides. A spring *r* imposes a counterclockwise force on the lever, holding it against its stop or against the lower end of the spindle *i*. The spring incorporated in the Ames

dial holds its spindle outward against the face of the end of the lever *l*.

The dial *o* has a lug on the back by which it is attached, by a screw *t* shown in Fig. 4 to a lug cast as an integral part of slide *m*. As previously suggested, this lug must be accurately machined to make the dial spindle exactly perpendicular to the slide guides at an exact distance from the pintle axis of the lever *l*.

The spindle *i* is designed with a wedge-shaped end for bearing against the inner side of the test bar, and a conical point at the other end which bears against the inner end of the lever *l*. The reduced diameter at the conical end produces a shoulder at that end, which strikes a corresponding shoulder in the spindle socket, and limits the movement of the spindle in that direction when the bar breaks. A small pin driven into the spindle near the wedge end moves freely in a slot in the wall of the spindle socket, preventing rotation of the spindle, and keeping the edge of the wedge-shaped end always perpendicular to the axis of the test bar. The spindle is case-hardened all over.

The various parts described work in train as follows:

Deflection of the test bar imparts motion to the spindle *i* which is held firmly against it by pressure of the inner face of the lever *l*, due to force of lever-spring *r*. A clockwise motion of the lever results, which moves the outer end away from the dial *o* an amount equal to the deflection of the test bar, the arms of the lever being of equal effective length. The interior spring of the dial *o* keeps its spindle pressed firmly against the face of the lever, the movement of which, equal to the deflection of the test bar, is thus measured and indicated by the dial. The perpendicular relationship between the dial spindle and the spindle *i*, conjoined with the same relationship between the faces of the two arms of the lever, and the fact that the planes of these faces pass through the lever pintle axis, results in equality between the amounts of motion of the two spindles, irrespective of the amount of angular displacement of the lever *l* from its initial position, as a simple diagram will immediately show.

Materials.—The device was constructed of steel throughout. The loading and weighing heads, *a* and *c*, and the slide *m* were cast, and the lever *l* hand-forged. The loading and supporting

pins are of tool steel, hardened and ground, and the spindle *i* and faces of the lever *l* were case-hardened and ground.

OPERATION.

In operation the test bar is held in place by hand until the loading pin grips it firmly with an initial force of 30 lb., which is about sufficient to crush through the sand layer adhering to the bar and produces no measurable deflection. The dial spindle is once for all adjusted so that when lever *l* is against its counterclockwise stop, some considerable further inward motion of the spindle is possible. The adjustment screw *s* is then turned so that the lever *l* is brought nearly but not quite to the extremity of its counterclockwise movement. The dial proper is now turned by means of the knurled ring which forms its mounting, so that the zero of the dial comes under the pointer. The operator then starts application of the load, checking the speed by a stop-watch, and the observer watches the dial and catches the deflection very easily at the moment of rupture.

Up to the time of writing about thirty bars have been tested with entirely satisfactory results, with the possible exception of the behavior of the spring *r*, which it is believed will be replaced by a short spiral spring, acting in tension on the dial arm of the lever and parallel to the dial spindle. A recent test of the accuracy of the deflectometer in place, using an inside micrometer against the wedge end of the spindle *i*, showed an apparent error of 0.0008 in. in a total movement of 0.1 in. Of this error probably half was due to flexure of the micrometer mounting. As has been noted in the description of the device, the design is such that the deflectometer is not subjected to any appreciable stress due to the stressing of the test bar, so that it is proper to assume that the error just noted is the maximum which will occur in actual use. This degree of accuracy is believed to be entirely sufficient for this character of test.

ENDURANCE AND IMPACT TESTS OF METALS.[1]

By D. J. McAdam, Jr.

SUMMARY.

This paper gives a description of some methods of endurance and impact testing developed at the U. S. Naval Engineering Experiment Station, Annapolis, Md.

The endurance test consists of alternating torsion applied to specimens of cylindrical test section and tapered fillets. Alternating torsional stress is applied to one end of the specimen by an arm which is given a reciprocal motion by an eccentric attachment to a rotating wheel. The stress is measured by the movement of two springs which resist the motion of an arm attached to the other end of the specimen.

Results obtained by this method indicate that this alternating torsion test will be of considerable use in investigation and testing of metals. The appearance of the surface of the specimen during test may reveal any lack of uniformity of the material under test. Curves plotted from the results show the relative endurance of various kinds of metal and the effects of various heat treatments. The relations between the endurance of metals, as determined by the alternating torsion test, and other physical properties are being determined.

The method of impact testing has given very consistent and interesting results. The method consists in the shearing of an unnicked specimen by impact applied by means of a swinging pendulum, and the measuring of the energy used in thus shearing the specimen.

This impact-shear method has three advantages over most methods of impact testing that are in use at present, namely: (1) it applies a single kind of stress instead of a combination of stresses; (2) unnicked specimens are used, thus removing the uncertainty due to the effect of the nick; and (3) one specimen can be moved along and sheared a number of times and the average results utilized.

[1] This paper is presented by permission of the Secretary of the Navy.—Ed.

ENDURANCE AND IMPACT TESTS OF METALS.[1]

By D. J. McAdam, Jr.

The following methods of endurance and impact testing have been originated at the U. S. Naval Engineering Experiment Station. Although sufficient time has not been available to compile a very complete series of results obtained with a great variety of metals, the experiments have been carried out far enough to show that the methods are of value.

A description of these methods, therefore, is given at this time, in the hope that it will prove of interest.

Alternating Torsion Test.

Description of Machine.—The testing of failed shafting and of forgings intended for use in shafting, led to the idea that endurance tests by means of alternating torsion would give results of considerable value. A satisfactory machine, however, for carrying out such tests was not available until one was invented by Mr. L. E. Foster, working in the metals laboratory of the station. This machine, a modification of the Upton-Lewis type, has been used in a number of tests at the station and has demonstrated its value in determining the serviceability of metals. The principles of the construction of the machine are illustrated in Fig. 1.

The arm *b*, caused to oscillate by means of its eccentric attachment to the rotating wheel *a*, is attached to one end of the specimen *c*. The rotation of the specimen is resisted by the opposing springs *ff* acting on the arm *e*, which is attached to the other end of the specimen *c*. The ends of the specimen are supported by the bearings *d*.

The torsional moment applied to the specimen is proportional to the strength of the springs and to their displacement, and to the length of the arm *e*. These values being measured, the nominal maximum stress on the specimen can be calculated. A record of movements of the springs, magnified by the indicator arm *g*, is marked on the revolving drum *h*.

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By using springs of different strengths and by varying the eccentric attachment on the wheel *a* (shown by scale *j*), the stresses applied to the test specimen may in turn be greatly varied. These stresses can always be calculated from the machine constants, strength of springs, and height of diagram marked on the drum *h*. By means of a counter and automatic stop the number of cycles necessary to break the specimen are recorded.

The size and shape of the specimen used are shown in

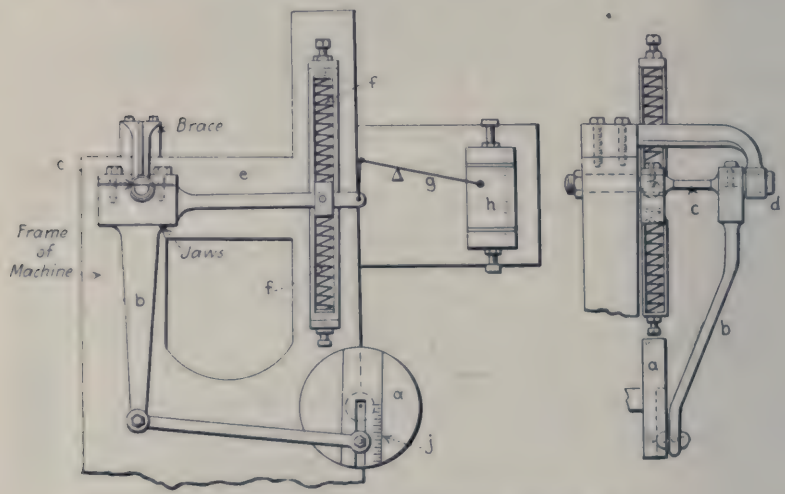

FIG. 1.—Machine for Alternating Torsion Test.

Fig. 2. The specimen is held rigidly by pressure of a flat piece of steel against the flat part of the specimen.

Results of Tests.—Although the experiments carried on with this machine have not progressed to the point where a series of results can be tabulated, the tests have shown the value of the method and have suggested numerous lines of investigations to be followed. Tests have been made with plain carbon steels, alloy steels of various composition, and with non-ferrous alloys.

In these tests, the observation of the material during test often proves as valuable in determining the quality of the metal as the numerical values obtained. Metal which contains slag

or other forms of segregation soon shows cracks extending along the lines of segregation, while uniform material usually shows no crack until it breaks transversely. In longitudinal specimens, the failure often starts by longitudinal cracks, while transverse specimens of the same material show no cracks until the final transverse break. Since a cylindrical test specimen $1\frac{1}{2}$ in. long and $\frac{1}{2}$ in. in diameter is used, plenty of opportunity is afforded to study the surface of the metal under test.

For a test of any kind of metal, at least four or five specimens are necessary. By adjusting the crank throw, the stress can be varied so that the number of cycles required for breaking will vary from a few to an unlimited number. A stress-cycle curve can then be plotted from the results obtained with all the specimens.

Results obtained with various kinds of steel are illustrated

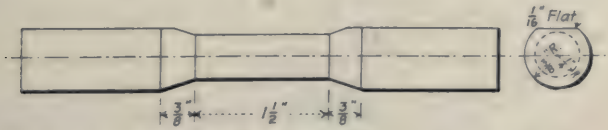

Fig. 2.—Specimen Used in Alternating Torsion Test.

by the curves shown in Fig. 3. Experiments are being made with various non-ferrous alloys, but the number of experiments is not yet great enough for compilation. In these curves, nominal maximum stresses are plotted as ordinates and the number of cycles to break as abscissas. Though the stresses are beyond the elastic limit, they are calculated by use of the same formulas that apply before the limit of elasticity has been reached. The stresses are, therefore, merely "nominal."

The lowest curve shown in Fig. 3 is that of ingot iron; the next higher curve is that of 0.3-per-cent carbon steel, Sample I. Curves *O-2* to *S-2* inclusive, are those of chrome-nickel steel which had received various heat treatments. The steels have the composition given in Table III. These values were furnished by the manufacturers.

The heat treatment of the chrome-nickel steel as given by the manufacturer was as follows: Quenched in oil from 1600° F., annealed at 1350° F.

As shown in Fig. 3, one specimen of chrome-nickel steel was tested as received, while five other specimens were heat-treated at the station before testing. The heat treatments are described in Table I. Frequently the first treatment followed by oil-quenching was omitted. In that case, the figure 2 is used after the letter designating the heat treatment.

It will be noticed that these heat treatments differ chiefly

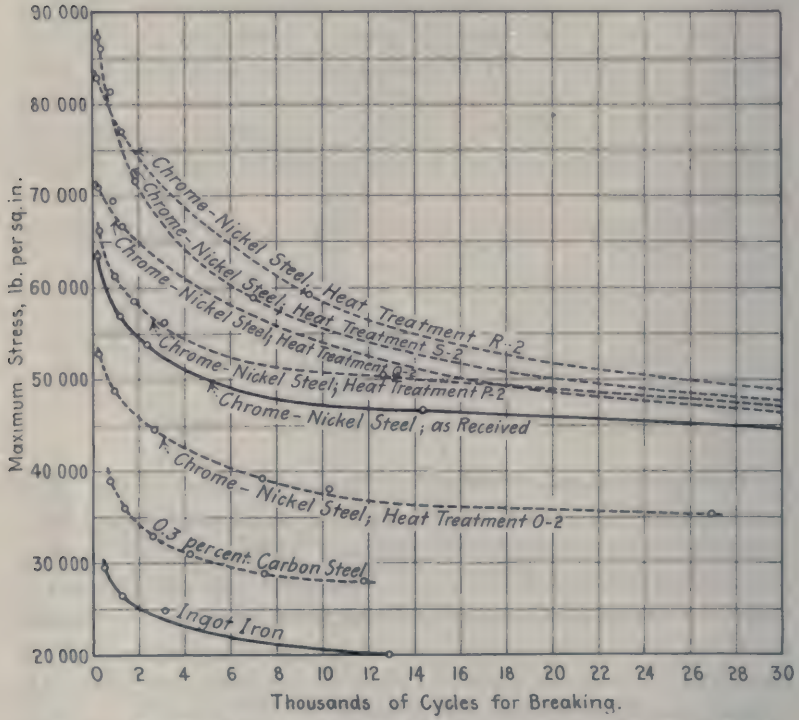
90 000
80 000
70 000
60 000
50 000
40 000
30 000
20 000
Maximum Stress, lb. per sq. in.
0 2 4 6 8 10 12 14 16 18 20 22 24 26 28 30
Thousands of Cycles for Breaking.
Chrome-Nickel Steel; Heat Treatment R-2
Chrome-Nickel Steel; Heat Treatment S-2
Chrome-Nickel Steel; Heat Treatment Q-2
Chrome-Nickel Steel; Heat Treatment P-2
Chrome-Nickel Steel; as Received
Chrome-Nickel Steel; Heat Treatment O-2
0.3 percent Carbon Steel
Ingot Iron

FIG. 3.—Results of Alternating Torsion Tests on Various Kinds of Steel.

in the temperature of final annealing or "drawing back." As this temperature is lowered, the curves become higher but their steepness is increased.

As the number of cycles is increased, the curves for specimens receiving heat treatments *Q-2*, *R-2 and S-2* soon drop down to or even below the curve representing heat treatment *P-2*. Points on curves representing treatments *R-2* and *S-2* were

obtained at about 100,000 cycles. Since, however, only a few results have been obtained at so great a number of cycles, the curves were not made on a sufficiently small scale to include these

Table I.—Heat Treatments of Steel.

Treatment Letter.	Heated to —° F., held 1 hr.	Cooled in	Heated to —° F., held ½ hr.	Cooled in	Heated to —° F., held ½ hr.	Cooled in
A	1700	Oil	1500	Furnace	....	
B	1700	Oil	1500	Oil	1100	Furnace
C	1700	Oil	1500	Oil	1000	Furnace
F	1750	Oil	1600	Furnace	....	
G	1750	Oil	1600	Oil	1100	Furnace
H	1750	Oil	1600	Oil	....	
I	1750	Oil	1600	Water	....	
J	1750	Oil	1650	Furnace	....	
K	1750	Oil	1650	Oil	1100	Furnace
L	1750	Oil	1650	Oil	....	
M	1750	Oil	1650	Water	....	
O	1700	Oil	1450	Furnace	....	
P	1700	Oil	1450	Oil	1300	Furnace
Q	1700	Oil	1450	Oil	1200	Furnace
R	1700	Oil	1450	Oil	1100	Furnace
S	1700	Oil	1450	Oil	1000	Furnace
T	1700	Oil	1450	Oil	900	Furnace
AA	1750	Oil	1550	Furnace	....	
AB	1750	Oil	1550	Oil	1300	Furnace
AC	1750	Oil	1550	Oil	1200	Furnace
AD	1750	Oil	1550	Oil	1100	Furnace
AE	1750	Oil	1550	Oil	1000	Furnace
AF	1750	Oil	1550	Oil	900	Furnace
AG	1750	Oil	1550	Oil	800	Furnace
AH	1750	Oil	1550	Oil	700	Furnace

results. The relative positions of these curves at stresses near the elastic limit have therefore not yet been determined.

Experiments along this line with this and other kinds of material are being carried on, and interesting results are expected.

TABLE II.—RESULTS OF IMPACT AND HARDNESS TESTS.

Material.	Kind of Specimen.	Treatment.	Impact: ft-lb. to Break.			Hardness.	
			First Specimen.	Second Specimen.	Average.	Brinell.	Sclero-scope.
Ingot Iron	.....	.....	37.6 – 37.9	35.1 – 37.1	36.9	.....	17.8
0.1-per-cent Carbon Steel[a]	.....	As received	35.1 – 35.1	34.6 – 35.3	35.05	98.6	16.1
	.....	J	35.1 – 36.1	35.5 – 37.0	35.95	90.8	15.3
	.....	K	37.9 – 37.9	39.7 – 40.2	38.95	95.8	16.3
	.....	L	37.9 – 37.9	38.8 – 39.0	38.4	95.4	17.0
	.....	M	33.2 – 35.2	39.2 – 39.7	36.85	109.6	19.4
0.2-per-cent Carbon Steel[a]	.....	As received	34.2 – 34.2	34.5 – 35.5	34.26	114.8	19.0
	.....	F	26.8 – 28.8	31.3 – 32.5	30.36	99.0	18.0
	.....	G	39.7 – 41.2	40.6 – 41.6	40.1	138.2	23.0
	.....	H	40.1 – 41.1	38.7 – 40.7	38.86	149.8	23.9
	.....	I	26.8 – 28.0	24.9 – 25.9	25.83	182.2	29.7
0.3-per-cent Carbon Steel; Sample I.	Longitudinal	As forged	30.5 – 32.5	.....	31.2	114.4	23.6
	Transverse	As forged	30.5 – 31.5	.....	31.1	.....	.....
	Longitudinal	A	30.0 – 31.5	.....	30.6	108.6	22.4
	Transverse	A	28.5 – 30.5	.....	29.4	.....	.....
	Longitudinal	B	39.7 – 39.7	.....	39.7	152.2	29.0
	Transverse	B	37.4 – 37.9	.....	37.7	.....	.....
0.3-per-cent Carbon Steel; Sample II.	Longitudinal	As received	34.2 – 34.2	34.2 – 34.7	34.35	98.2	22.7
	Longitudinal	AA	35.1 – 36.1	34.1 – 35.1	35.25	97.4	19.5
	Longitudinal	AB	44.3 – 44.5	44.5 – 45.3	44.8	120.2	23.3
	Longitudinal	AC	45.2 – 45.7	.....	45.5	126.0	24.0
	Longitudinal	AD	43.8 – 44.3	42.9 – 43.6	43.7	137.6	25.3
	Longitudinal	AE	41.5 – 41.5	.....	41.5	140.0	24.1
	Longitudinal	AF	41.5 – 42.5	41.5 – 42.5	41.8	134.6	25.0
	Longitudinal	AG	40.6 – 40.6	.....	40.6	134.6	27.5
	Longitudinal	AH	40.6 – 41.1	.....	40.8	137.0	25.0
0.5-per-cent Carbon Steel.	Longitudinal	As forged	30.9 – 31.7	31.0 – 32.5	32.0	146.6	29.2
	Transverse	As forged	30.1 – 30.6	29.6 – 31.6	30.5	.....	.....
	Longitudinal	A	28.7 – 29.7	28.7 – 29.7	29.1	134.0	28.4
	Transverse	A	28.2 – 29.2	27.3 – 28.8	28.4	.....	.....
	Longitudinal	B	34.2 – 35.2	35.6 – 36.1	35.35	179.8	32.4
	Transverse	B	34.6 – 35.1	34.2 – 34.7	34.7	.....	.....
Nickel Steel; Sample I.	Longitudinal	As forged	34.6 – 35.6	35.1 – 35.6	35.2	156.6	29.2
	Transverse	As forged	31.9 – 32.6	30.5 – 32.0	31.8	.....	.....
	Longitudinal	A	32.7 – 33.5	32.4 – 32.9	32.85	152.2	24.6
	Transverse	A	32.4 – 33.4	30.4 – 32.1	32.08	.....	.....
	Longitudinal	B	31.9 – 32.7	32.4 – 32.4	32.45	183.0	34.2
	Transverse	B	30.6 – 31.6	31.0 – 32.0	31.3	.....	.....
Nickel Steel; Sample II.	Longitudinal	As received	33.3 – 34.3	.....	33.8	156.6	27.8
	Longitudinal	A-2	31.9 – 32.9	.....	32.4	145.4	25.5
	Longitudinal	C-2	27.8 – 28.1	.....	28.0	217.0	43.4
Chrome-Nickel Steel.	Longitudinal	As received	37.1 – 38.4	38.6 – 38.8	38.25	172.4	30.6
	Transverse	As received	33.5 – 34.3	34.2 – 34.7	34.18	.....	.....
	Longitudinal	O-2	35.3 – 36.3	.....	35.3	165.4	27.5
	Transverse	O-2	34.5 – 35.2	34.5 – 35.2	34.85	.....	.....
	Longitudinal	P-2	35.3 – 35.7	37.0 – 37.2	36.73	166.0	29.8
	Longitudinal	Q-2	30.1 – 30.6	28.7 – 29.2	29.75	193.6	33.1
	Longitudinal	R-2	25.5 – 25.9	25.9 – 26.1	25.85	225.0	40.5
	Transverse	F-2	26.8 – 27.3	26.8 – 26.8	26.85	230.8	43.6
	Transverse	S-2	23.5 – 23.8	23.3 – 23.5	23.5	230.8	44.3
	Transverse	T-2	21.2 – 22.1	22.1 – 23.4	22.35	257.8	46.6
Monel Metal	Cylindrical bar, 1 in. diam.	Hot-rolled	41.3 – 41.6	42.5 – 42.5	42.0	.....	.....
Copper	Cylindrical bar, 1 in. diam.	Cold-rolled	25.4 – 25.4	.....	25.4	.....	.....
	Cylindrical bar, 1 in. diam.	Rolled and annealed	23.5 – 25.0	24.0 – 24.0	24.1	.....	.....

[a] 1-in. rivet steel.

Table II.—Results of Impact and Hardness Tests (*Continued*).

Material.	Kind of Specimen.	Treatment.	Impact: ft-lb. to Break.			Hardness.	
			First Specimen.	Second Specimen.	Average.	Brinell.	Sclero-scope.
Phosphor-Bronze	Cylindrical bar, 1 in. diam...	Cold-finished	34.5 - 35.2	34.2 - 34.2	34.6	.....	....
	Cylindrical bar, 1 in. diam..	Annealed....	34.2 - 35.7	.....	35.0	.....	....
Muntz Metal..	Cylindrical bar, 1 in. diam..	Extruded....	25.4 - 25.9	.....	25.7	.....	....
	Cylindrical bar, 1 in. diam..	Extruded and cold-drawn	26.8 - 28.8	.....	26.8	.....	....
Naval Brass; Sample I.	Cylindrical bar, 1 in. diam ..	Extruded....	24.0 - 24.5	.....	24.2	.....	....
	Cylindrical bar, 1 in. diam..	Extruded and cold-drawn	24.9 - 28.9	.....	25.4	.....	....
Naval Brass; Sample II.	Cylindrical bar, 1 in. diam..	Rolled.....	21.1 - 23.1	.....	22.1	.....	....
Naval Brass; Sample III.	Cylindrical bar, 1 in. diam ..	Rolled.....	24.9 - 26.3	.....	25.8	.....	....
Manganese Bronze; Sample I.	Cylindrical bar, 1 in. diam ..	Extruded....	25.8 - 28.8	.....	26.9	.....	....
	Cylindrical bar, 1 in. diam ..	Extruded and cold-drawn	22.8 - 23.3	.....	23.1	.....	....
Manganese Bronze; Sample II.	Cylindrical bar, 1 in. diam ..	Extruded....	23.3 - 25.2	.....	24.4	.....	....
	Cylindrical bar, 1 in. diam ..	Extruded and cold-drawn	22.5 - 23.5	.....	23.0	.....	....
Aluminum-Copper-Zinc Alloy.	Cylindrical bar, 1 in. diam ..	Cold-rolled..	7.8 - 8.3	.....	8.1	.....	....
Composition "G"	Cylindrical bar, 1 in. diam ..	Cast.....	22.5 - 25.0	.....	23.5	.....	....
Composition "M"	Cylindrical bar, 1 in. diam ..	Cast.....	19.6 - 24.1	.....	21.5	.....	....

A comparison of the results shown in Fig. 3 with the results of impact tests on the same material, recorded in Table II, is of interest.

Impact Tests.

Description of Machine.—A method of impact testing has been developed at this station, which it is believed is free from many of the disadvantages of methods previously used. So far as can be learned by the writer this method has not been previously used.

The method consists in the shearing of an unnicked speci-

men by impact; and the measuring of the energy thus utilized. Although this impact-shear could be applied by various methods, it was applied in these experiments by means of a swinging pendulum as in other well-known methods now in use. The

TABLE III.—CHEMICAL COMPOSITION OF STEEL SPECIMENS.

	Carbon, per cent.	Sulfur, per cent.	Phosphorus, per cent.	Manganese, per cent.	Silicon, per cent.	Nickel, per cent.	Chromium, per cent.
0.3-per-cent Carbon Steel; Sample I	0.35	0.024	0.013	0.54	0.142	....	...
0.3-per-cent Carbon Steel; Sample II	0.274	.....	.....	0.64	.....	....	...
0.5-per-cent Carbon Steel	0.50	0.022	0.014	0.56	0.161	....	...
Nickel Steel; Sample I	0.35	0.026	0.018	0.64	0.150	3.47	...
Nickel Steel; Sample II	0.33	....	.....	0.64	....	3.25	...
Chrome-Nickel Steel	0.30	.....	....	1.60	....	3.50	0.7

residual energy in the pendulum is measured by the height to which the pendulum swings after the specimen is broken.

The general appearance of the apparatus used is shown in Fig. 4 (*a*). The pendulum *a* swings so that the horizontal edge

TABLE IV.—CHEMICAL COMPOSITION OF NON-FERROUS METAL SPECIMENS

	Copper, per cent.	Aluminum, per cent.	Zinc, per cent.	Tin, per cent.	Nickel, per cent.	Iron, per cent.	Manganese, per cent.	Lead, per cent.	Silicon, per cent.	Phosphorus, per cent.
Monel Metal	30.15	....	....	....	65.40	2.31	1.91	nil	0.15	....
Copper	99.96	.....	nil	nil	....	0.03	....	nil	.....	....
Phosphor-Bronze	95.94	....	trace	3.69	....	trace	....	nil	.....	0.26
Aluminum Alloy	2.04	87.60	10.02	nil	....	0.06	....	nil	0.289	....
Composition "G"	88.14	....	1.70	10.06	....	trace	....	trace	....	....
Composition "M"	87.5	....	3.88	7.62	....	trace	....	0.92	....	....

of the piece *e* just clears the upper surface of the holders *b b*. The vertical edge of the piece *e* consequently strikes against the specimen *c*, which is thus sheared off. The residual energy is measured by the distance through which the pointer *d* swings.

An enlarged view of the pendulum, specimen and holders

is shown in Fig. 4 (*b*). In this view, the vertical surface of the piece *e* is shown as it strikes against the specimen *c* and just clears the holders *b b*. About $\frac{1}{64}$ in. clearance is used.

It was found that careful adjustment of the shearing edge was necessary, so that "clearance" could be obtained, as in an ordinary cutting tool. Before this fact was realized, the values obtained were too high. The general order of results, however, remain the same.

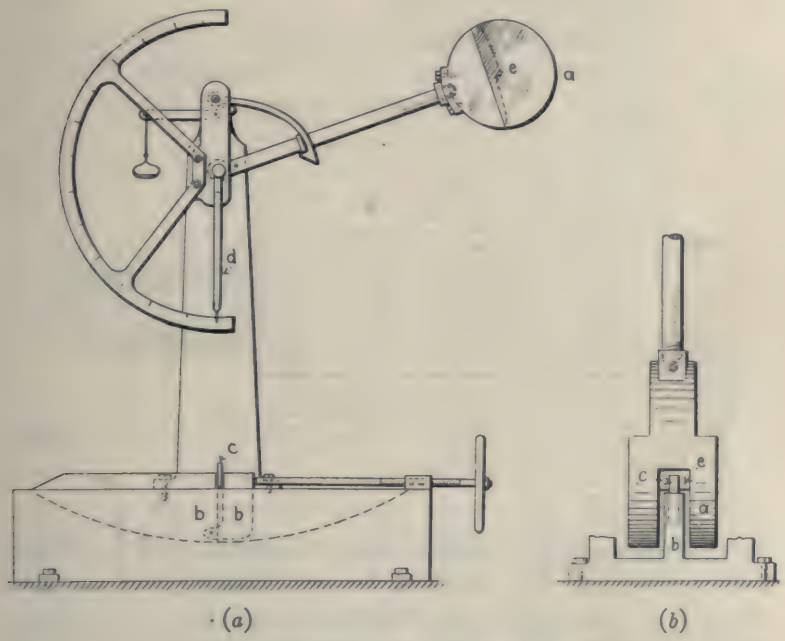

Fig. 4.—Apparatus for Impact Test.

A plan view showing the dimensions of the test specimen, and the manner in which it is held, is shown in Fig. 5.

Results of Tests.—Although various kinds of steel were used in these tests, the specimen, $\frac{3}{8}$ by $\frac{1}{4}$ in. in cross-section, was always sheared off so as to give a smooth cut. The piece cut off showed only slight deformation.

One advantage of the use of unnicked specimens is that the same specimen about $1\frac{1}{2}$ in. long, can be raised after each

cut and thus sheared several times. With uniform material, very consistent results were obtained in this way. The close agreement in results from the same specimen can be seen by reference to Table II.

A series of heat treatments of the various kinds of steel have been made, and impact-shear results obtained from this material. A number of other metals have also been tested by the same method. The results are all given in Table II. A series of hardness, tension and torsion tests, made on the same material, are given in Tables II, V, and VI.

By a comparison of these four tables it will be seen that there is no easily discovered relation between the results of impact tests and those of tension, torsion and hardness tests.

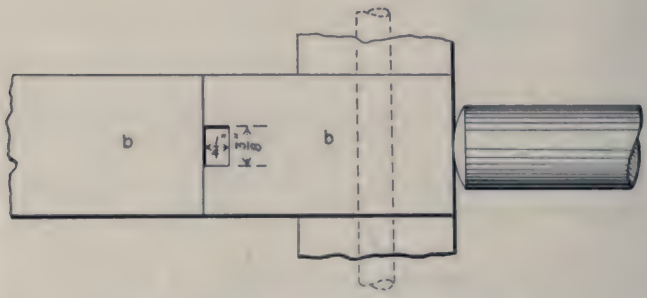

FIG. 5.—Specimen Used in Impact Test.

For example, as the "drawing back" temperature of heat treatment is lowered, the hardness, tensile strength and torsional strength of the metal rise continuously. The impact-shear results, however, soon reach a maximum, in the steels having more than 0.2 per cent carbon, and then decline more or less rapidly. Moreover the impact results do not vary directly as the ductility alone. This is well illustrated by the results obtained with annealed material. Evidently the impact-shear test is of value in measuring that combination of strength and ductility known as "toughness."

The results obtained with medium-carbon steels in the sorbitic condition (oil-quenched and tempered), and ingot iron, range from 35 to 45 ft-lb. The same steels in the annealed

Table V.—Results of Tension Tests.

Kind of Material.	Specimen No.	Kind of Specimen.	Treatment.	Tensile Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Proportional Limit, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.
Ingot Iron	4501	.....	As received..	43 370	24 500	22 500	43.0	76.1
0.1-per-cent Carbon Steel.[a]	6036	.....	As received..	53 870	36 000	34 500	34.5	70.5
	6019	.....	J	51 250	34 500	33 000	41.5	71.5
	6025	.....	K	58 870	38 000	37 500	40.0	74.0
	6018	.....	L	58 750	45 500	45 000	39.0	70.5
	6007	.....	M	68 000	52 200	49 000	26.5	72.5
0.2-per-cent Carbon Steel.[a]	5036	.....	As received..	64 250	37 500	37 000	32.0	61.5
	5025	.....	F	70 000	33 000	32 500	29.0	44.5
	5022	.....	G	78 000	45 500	45 000	29.5	63.5
	5014	.....	H	99 900	55 500	52 500	19.0	38.0
	5008	.....	I	111 250	70 500	46 500	9.0	20.0
0.3-per-cent Carbon Steel; Sample I.	8	Longitudinal..	As forged...	72 000	34 500	29 500	34.0	47.0
	46	Transverse....	As forged...	72 250	32 500	29 500	26.5	45.5
	183	Longitudinal..	A	69 500	36 000	34 000	31.5	48.5
	46	Transverse....	A	68 750	36 000	34 000	27.0	45.5
	14	Longitudinal..	B	87 750	54 000	53 500	27.0	60.0
	107	Transverse....	B	88 500	52 500	49 000	24.0	49.5
0.3-per-cent Carbon Steel; Sample II.	14	Longitudinal..	As forged...	61 500	23 500	20 500	36.0	59.0
	8	Longitudinal..	AA	63 000	31 000	31 000	36.5	60.0
	18	Longitudinal..	AB	68 590	43 720	43 220	33.0	71.5
	137	Longitudinal..	AC	72 590	43 650	39 000	31.0	70.0
	86	Longitudinal..	AD	76 250	49 000	47 500	32.0	65.5
	140	Longitudinal..	AE	76 000	49 000	47 500	31.0	67.5
	139	Longitudinal..	AF	78 750	46 500	46 000	28.0	65.0
	101	Longitudinal..	AG	80 000	47 500	41 500	28.5	66.0
	138	Longitudinal..	AH	79 590	50 000	46 940	29.0	65.3
0.5-per-cent Carbon Steel.	488	Longitudinal..	As forged...	88 000	37 500	32 500	23.0	38.5
	471	Transverse....	As forged...	87 250	37 000	32 500	17.5	15.0
	496	Longitudinal..	A	82 500	40 500	38 500	26.5	38.5
	317	Transverse....	A	80 750	39 000	37 200	20.5	24.5
	497	Longitudinal..	B	104 000	63 000	56 500	22.5	50.5
	312	Transverse....	B	104 000	63 000	59 500	18.5	32.5
Nickel Steel; Sample I.	635	Longitudinal..	As forged...	91 500	52 500	47 500	25.0	54.5
	619	Transverse....	As forged...	91 750	51 500	46 500	16.0	40.5
	797	Longitudinal..	A	86 750	51 000	49 000	28.5	52.5
	784	Transverse....	A	87 750	49 500	47 500	24.0	36.0
	636	Longitudinal..	B	104 000	76 500	76 000	24.5	64.0
	620	Transverse....	B	111 250	87 000	82 000	15.5	31.5
Nickel Steel; Sample II.	537	Longitudinal..	As received..	95 250	53 500	49 000	23.5	51.5
	535	Longitudinal..	A-2	89 250	58 500	57 000	27.0	53.5
	510	Longitudinal..	C-2	120 500	94 000	89 500	18.5	56.5
Chrome-Nickel Steel.	3521[b]	Longitudinal..	As received..	106 000	58 000	49 000	13.4[c]	57.0
	3652	Transverse....	As received..	108 950	62 500	53 500	21.5	41.0
	3522[b]	Longitudinal..	O-2	92 000	52 500	47 500	19.1[c]	61.5
	3639	Transverse....	O-2	95 250	48 000	46 500	22.0	34.5
	3513[b]	Longitudinal..	P-2	108 250	77 500	73 000	14.6[c]	61.5
	3514[b]	Longitudinal..	Q-2	115 500	100 000	94 000	11.1[c]	62.5
	3519[b]	Longitudinal..	R-2	132 000	120 500	112 500	8.5[c]	57.0
	3517	Transverse....	F-2	127 500	109 500	100 500	23.5	28.5
	3515	Transverse....	S-2	136 500	130 000	121 000	11.0	21.0
	3516	Transverse....	T-2	154 750	143 500	133 000	4.5	8.0

[a] 1-in. rivet steel.
[b] 8-in. specimen.
[c] Elongation in 8 in.

TABLE V.—RESULTS OF TENSION TESTS (*Continued*).

Kind of Material.	Specimen No.	Kind of Specimen.	Treatment.	Tensile Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Proportional Limit, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.
Monel Metal	201[b]	Cylindrical bar, 1 in. diam.	Hot-rolled	92 500	64 000	58 000	24.3[c]	69.5
Copper	481	Cylindrical bar, 1 in. diam.	Cold-rolled	32 150	14 000	11 000	51.0	71.0
	541	Cylindrical bar, 1 in. diam.	Annealed	33 100	15 000	11 000	44.0	66.0
Phosphor-Bronze.	41[b]	Cylindrical bar, 1 in. diam.	Cold-finished.	118 750	50 500	32 500	12.1[c]	74.0
	101[b]	Cylindrical bar, 1 in. diam.	Annealed	45 060	15 000	12 500	53.1[c]	80.5
Muntz Metal	661	Cylindrical bar, 1 in. diam.	Extruded	51 870	13 000	13 000	54.5	61.0
	721	Cylindrical bar, 1 in. diam.	Extruded and cold-drawn	57 870	29 500	26 500	46.0	65.5
Naval Brass; Sample I.	821	Cylindrical bar, 1 in. diam.	Extruded	64 310	17 520	16 020	39.0	40.9
	881	Cylindrical bar, 1 in. diam.	Extruded and cold-drawn	65 040	25 060	22 050	37.5	37.75
Naval Brass; Sample II	1422	Cylindrical bar, 1 in. diam.	Rolled	45 500	32 000	19 000	38.0	47.0
Naval Brass; Sample III	741	Cylindrical bar, 1 in. diam.	Rolled	65 500	23 000	18 000	39.5	43.5
Manganese Bronze; Sample I.	1001	Cylindrical bar, 1 in. diam.	Extruded	72 870	26 500	19 000	32.0	35.0
	1061	Cylindrical bar, 1 in. diam.	Extruded and cold-drawn	77 000	46 000	26 500	30.0	40.5
Manganese Bronze; Sample II.	1221	Cylindrical bar, 1 in. diam.	Extruded	70 000	23 500	19 000	39.5	51.0
	1241	Cylindrical bar 1 in. diam.	Extruded and cold-drawn	73 070	34 030	25 020	32.5	46.4
Aluminum-Copper-Zinc Alloy	241[b]	Cylindrical bar, 1 in. diam.	Cold-rolled	46 190	38 070	34 010	8.6[c]	41.0
Composition "G"	423	Cylindrical bar, 1 in. diam.	Cast	34 000	16 000	12 000	15.5	18.0
Composition "M"	404	Cylindrical bar, 1 in. diam.	Cast	31 500	14 000	11 000	15.5	21.0

[b] 8-in. specimen.
[c] Elongation in 8 in.

Table VI.—Results of Torsion Tests.

Specimens ¾ in. in Diameter, 3 in. Test Length.

Kind of Material.	Kind of Specimen.	Treatment.	Shearing Stress at Proportional Limit, lb. per sq. in.	Nominal Shearing Stress at Maximum Torque, lb. per sq. in.	Angle of Twist at Proportional Limit, minutes per linear inch.	Angle of Twist at Break, degrees per linear inch.
0.3-per-cent Carbon Steel; Sample I.	Longitudinal..	As forged...	12 070	57 490	9.3	152.6
	Transverse....	As forged...	14 490	57 490	10.0	180.0
	Longitudinal..	A	19 320	55 000	13.3	187.3
	Transverse....	A	16 900	53 650	13.3	136.6
	Longitudinal..	B	24 140	68 810	16.7	295.6
	Transverse....	B	24 140	60 040	16.7	235.6
0.3-per-cent Carbon Steel; Sample II.	Longitudinal..	As received..	18 110	52 150	14.8	236.9
	Longitudinal..	AA	21 730	52 240	17.3	263.6
	Longitudinal..	AB	24 140	59 850	18.8	478.3
	Longitudinal..	AC	27 760	61 300	22.2	443.0
	Longitudinal..	AD	29 320	60 660	22.7	395.0
	Longitudinal..	AE	33 800	59 760	28.3	260.0
	Longitudinal..	AF	26 600	61 750	20.0	490.8
	Longitudinal..	AG	36 220	66 730	30.0	327.3
	Longitudinal..	AH	31 390	64 740	25.0	366.3
0.5-per-cent Carbon Steel.	Longitudinal..	As forged...	16 900	65 420	12.7	158.3
	Transverse....	As forged...	16 900	63 380	12.7	105.0
	Longitudinal..	A	26 560	62 470	19.3	153.3
	Transverse....	A	21 730	60 660	16.0	141.6
	Longitudinal..	B	41 040	77 860	30.3	256.6
	Transverse....	B	43 460	73 790	30.0	154.3
Nickel Steel; Sample I.	Longitudinal..	As forged...	28 970	71 070	20.7	243.3
	Transverse....	As forged...	21 730	67 450	15.0	150.0
	Longitudinal..	A	16 900	67 650	11.7	207.6
	Transverse....	A	33 800	66 090	23.3	146.6
	Longitudinal..	B	41 040	74 920	32.3	280.0
	Transverse....	B	53 120	75 330	40.0	143.3
Nickel Steel; Sample II.	Longitudinal..	As received..	28 970	71 530	22.5	240.0
	Longitudinal..	A-2	36 220	63 090	28.7	195.0
	Longitudinal..	C-2	65 850	85 100	50.0	235.0
Chrome-Nickel Steel.	Longitudinal..	As received..	19 320	74 240	16.0	160.0
	Transverse....	As received..	16 900	76 690	13.3	115.0
	Longitudinal..	O-2	26 560	72 610	22.0	229.6
	Transverse....	O-2	24 140	70 085	18.3	168.3
	Longitudinal..	P-2	43 460	84 200	31.7	212.6
	Longitudinal..	Q-2	53 120	80 580	40.0	261.6
	Longitudinal..	R-2	77 260	89 180	56.0	210.0
	Transverse....	G-2	65 190	84 200	51.7	150.0
	Transverse....	S-2	82 090	90 990	62.7	95.0
	Transverse....	T-2	86 920	100 500	67.7	101.6
Monel Metal.....	Longitudinal..	Hot-rolled..	38 630	72 160	34.0	742.6
Copper.....	Longitudinal..	Cold-rolled..	8 450	30 600	12.3	738.3
	Longitudinal..	Annealed...	2 410	31 600	5.7	744.3
Phosphor-Bronze.	Longitudinal..	Cold-finished	33 800	52 970	48.0	691.6
	Longitudinal..	Annealed...	9 660	47 310	12.3	725.0

condition give lower results by as much as 10 ft-lb., depending largely on the carbon content.

The results obtained with the two samples of nickel steel were somewhat surprising. Neither annealing nor oil-quenching and tempering gave results equal to those obtained with plain carbon steels, although the tension and torsion results were greatly improved by the latter treatment. Annealing gave slightly better results than quenching and tempering. Since it is possible that the samples used are not entirely representative, further experiments will be made before final conclusions are drawn.

The results with copper and its alloys are also surprising. With the exception of phosphor bronze, all the alloys of copper gave results differing only slightly among themselves. The cold-worked material having high tensile strength, gave results almost identical with those of annealed material having much lower tensile strength and greater ductility. Cast material gave almost the same results as rolled material. Evidently the impact properties of copper alloys depend not so much on mechanical treatment as on methods of melting and casting. Further work, however, must be done along this line before these properties are well understood.

It will be noticed that results obtained by this impact test do not extend over a very wide range of numerical values. The values, however, differ as much as many of the values obtained in tension tests. Consistent results, moreover, even if they differ only slightly among themselves, are of more value than inconsistent, widely varying results.

It is desirable that further experiments be made, using a machine designed for adjustable clearance and capable of using varying amounts of total energy in the breaking of the specimen. A machine having greater total energy is also desirable, so that larger specimens can be used without diminishing the excess energy necessary for a successful impact test.

DISCUSSION.

Mr. H. B. Allen (*by letter*).—It is splendid to see the increasing number of papers written on the application of such comparatively new methods of testing as those described in Mr. McAdam's paper. A great deal of work done in this country on such tests is not made public. This is regrettable, as it delays the standardization of the tests and their usefulness. In this respect the Europeans are considerably ahead of us. Mr. Allen.

Mr. McAdam is to be congratulated upon the work he has done and the ground he has covered. But here again are purely comparative figures, which I, for instance, cannot compare with my own results, although I make tests called by the same names.

However, all work along these lines is leading in the right direction; namely, showing the usefulness and application of the tests. When manufacturers and consumers realize that the use of such tests is of considerable advantage to them, then more earnest efforts to standardize them will be made.

The writer has used the impact and endurance tests for some time on high-grade carbon and alloy tool steels and on the tools made from these steels, and has been able to demonstrate their usefulness. The impact tests are made on a machine of similar design to that used by Mr. McAdam, except that sufficient clearance is allowed between the anvil and top to eliminate the shearing effect to a great extent, thus giving the more conventional type of impact test. A nicked specimen is used. The nick is easily cut by either a hacksaw or an emery disk, is $\frac{1}{16}$ in. deep, and is found to give very consistent results. The specimen is made of any size up to $\frac{3}{8}$ in. square. Unfortunately the results of tests on specimens of different size can not as yet be reduced to stress per unit area. A standard must therefore be fixed for each product tested.

To cite a single application of the impact test in our work, we will take a test made on a finished product in the shape of a steel plate about $\frac{1}{8}$ in. in thickness. The material was 0.80–0.90-per-cent carbon crucible steel and had been hardened and tem-

Mr. Allen. pered to give a Brinell hardness number of about 360. Two plates of the same analysis and heat treatment were taken, one plate showing up better in work than the other. The Brinell and scleroscope hardnesses of each were the same. Under ordinary microscopic examination the structure of each was the same. The ultimate tensile strength of each was about the same, approximately 200,000 lb. per sq. in. But the impact test showed one plate to be 100 per cent tougher than the other.

This led to a much more searching microscopic investigation which furnished the clue. The trouble was found and corrected in the annealing before hardening.

Mr. McAdam. MR. D. J. MCADAM, JR. (*Author's closure by letter*).—I agree with Mr. Allen that methods of impact testing should be standardized so that the results obtained by different observers will be comparable.

My experience with the method used by Mr. Allen is that it does not give consistent results with most varieties of steel. With medium and low-carbon steels the specimen sometimes breaks straight across; very often, however, it tears diagonally. Such variations in breakage cannot give consistent results.

With tool steels, it is possible that more consistent results may be obtained. It seems evident, however, that nicked specimens, even when the nick is very accurately made, cannot give as consistent results as unnicked specimens. The effect of the nick would be especially noticeable when thin specimens are used.

AN APPARATUS FOR DETERMINING SOIL PRESSURES.

By A. T. Goldbeck and E. B. Smith.

SUMMARY.

This paper describes an apparatus for measuring the pressure under earth fills or against walls. A small cell having a thin brass, annular diaphragm is buried at the desired position with pipe and electrical connections to an air supply and electrical equipment. Air pressure within the cell equilibrates the external soil pressure as indicated by breaking electrical contact, and the pressure is read on a pressure gage. Typical calibration results are given.

AN APPARATUS FOR DETERMINING SOIL PRESSURES.

By A. T. Goldbeck and E. B. Smith.

For many hundreds of years, engineering structures have been built whose safety to a large extent has been dependent on the physical behavior of the adjoining or the underlying soil, and although these centuries have elapsed with their records of disastrous failures, our ideas of the laws governing the action of soils and the pressures which they exert are still indefinite, and are based mainly on mathematical theory rather than on adequate experimental data. Certainly the present vagueness of our knowledge is not due to any lack of effort, for many attempts at an experimental solution are recorded in engineering literature. The very fact that there are now no satisfactory laws concerning earth pressures evidences the inadequacy of the existing data for the formulation of such laws, and is suggestive of the difficulty and elusiveness of the problem.

Measurements of the pressure of earth against or under a wall or other structure are rendered difficult, because the location at which the measurement should be made is almost impossible of access without disturbing the soil, and such measurements have not been accomplished because no apparatus seems to have been designed for this use, whose action does not have some influence on the very pressure it was constructed to measure.

Then, too, the many variations in the condition and character of fills make the problem very complicated. It has been the aim of the authors to develop an apparatus that will be applicable to the measurement of earth pressures against structures, as well as for use in the laboratory for the study of the laws of pressure distribution through particular kinds of materials, under different conditions of compaction, moisture content, load application, etc.

Soils are made up of granular particles of various sizes, together with very finely divided material, generally containing

moisture. The action of pressures is to cause motion of some of these particles with respect to others, and the manner of transmission of pressures is influenced by the mobility of the mass or the ease with which the particles move with respect to one another. It is evident that several factors must control this movement, such as cohesion and friction, and these factors are in turn governed by the physical characteristics of the soil, such as the grading, moisture content, kind of fine material, degree of compaction, etc. It is probable that all of these characteristics should be known so as to make any experimental data obtained applicable to similar cases in the future. In view of the fact that friction and cohesion between the particles exist, it is evident that an apparatus employed for the measurement of soil pressures should be so constructed that no movement of its parts will take place against or away from the soil during the measuring process. If the measurement requires a movement against the soil, a higher pressure than actually exists will be indicated. On the other hand, should a movement away from the soil be necessary, an indication lower than the true pressure will be obtained. The authors have therefore attempted to develop an apparatus which will measure the pressure in any direction of an earth fill with practically no disturbance of its natural condition, and in considering such an apparatus it was deemed advisable to develop the idea of a portable cell of small size, capable of indicating at some remote station the value of any pressure coming upon it.

DESCRIPTION OF APPARATUS.

After a great deal of experimenting, a diaphragm cell has been developed as shown in Fig. 1. A cast-iron base *G* is fitted with a brass diaphragm *J*, held in place by the annular ring *C*. On each side of this diaphragm are disks, *A* and *B*, securely and rigidly held together by means of Bakelite cement and machine screws, as shown. These plates have an annular clearance from the ring *C* of 0.03 in., thus allowing only a small annular portion of the diaphragm *J* to be the flexible element. The diaphragm *H* serves the double purpose of protection and of stiffening the plates *A* and *B* against side motion and eccentric loads. In the

base G is placed a slightly crowned contact support F, held in place and electrically insulated from the base by Bakelite cement.

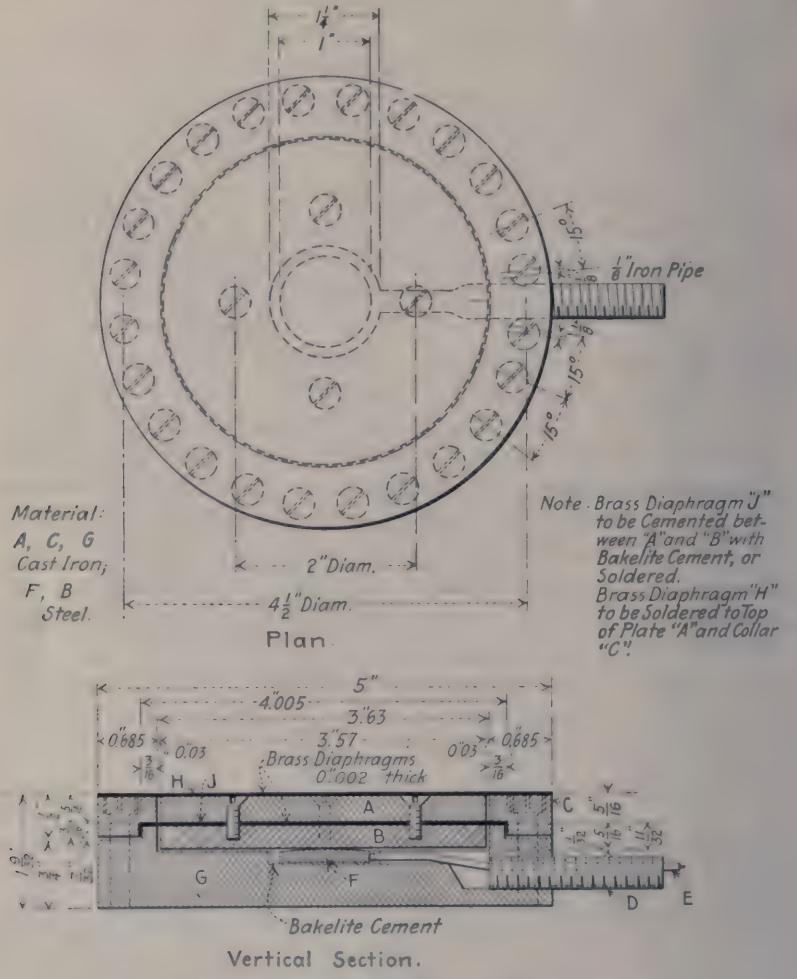

FIG. 1.—Diaphragm Cell for Determining Soil Pressure.

An insulated bell wire E is soldered to the support F, and passes to the outside connections through the $\frac{1}{8}$-in. pipe d.

In the use of this apparatus the cell is buried in the soil to

the required depth, and, by means of the $\frac{1}{8}$-in. pipe and the bell wire, is connected to the remote control and indicating instruments as shown diagrammatically in Fig. 2. Air pressure is slowly

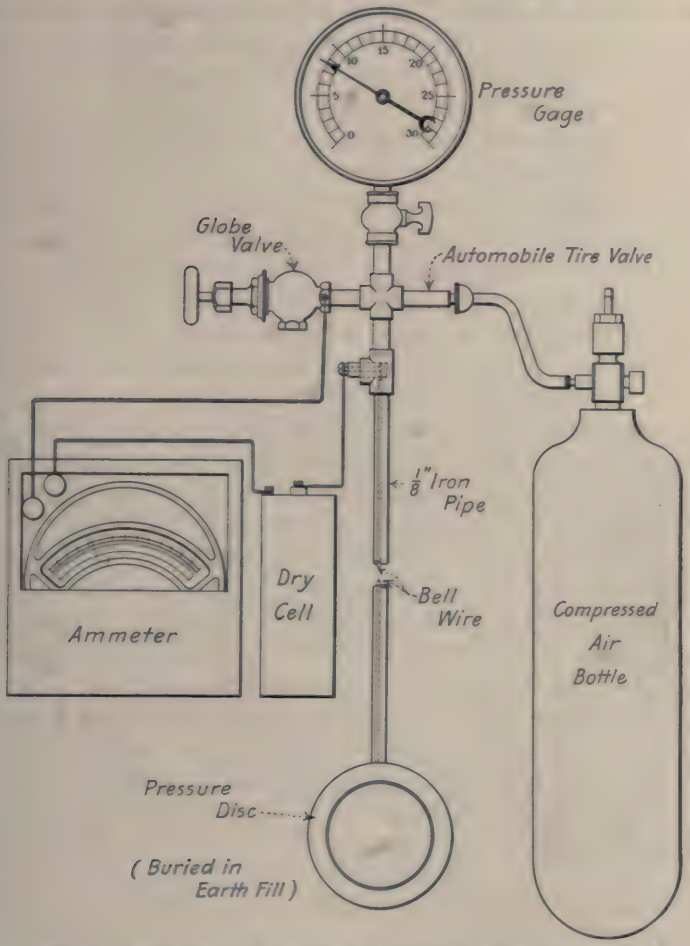

FIG. 2.—Control and Indicating Apparatus for Determining Soil Pressure.

admitted from a compressed-air supply until the air pressure within the cell equilibrates the external pressure on the disk *A* (Fig. 1), and causes the contact between disk *B* and support *F* to be broken, as indicated by an ammeter or a telephone receiver.

In the development of this cell an attempt was made to reduce the movement of the disks and diaphragm to the very smallest possible value, for as previously mentioned, any movement of the disks against the soil fill introduces stress conditions which will no doubt vitiate the results. It was found that a movement of 0.001 in. or more against the soil fill required a very large increase in load.

Pressures of very small values on the diaphragm disks were found to deform them appreciably, and a correspondingly greater movement was necessary to secure a pressure reading. This elastic deformation existed in the disks, the contact support, and in the insulating material. In the construction of the first experimental cells, brass was used throughout in combination with such insulating materials as paper, shellac, mica, celluloid, and hard fiber. The joints on the diaphragm between the disks *A* and *B* were made tight by pulling the disks together with twelve No. 10 machine screws, the surfaces having been coated with a special compound of rubber, beeswax, and vaseline. But none of these combinations was found satisfactory as the elastic deformations varied from 0.005 in. to over 0.01 in.

By the adoption of cast-iron and steel in the construction of these cells, with the use of Bakelite as a cement and insulator under the contact support, and also for cementing the disks to the diaphragm, and by reducing the deforming length of the loaded parts, the elastic deformation has been reduced to about 0.0003 in. The movement necessary to break the electrical contact is not over 0.00001 in. With the present construction and type of cell it has been found easy to obtain check readings on wet and dry sand and on damp clay, without appreciably changing the load on the material.

CALIBRATION OF PRESSURE DISK.

In order to test the accuracy of the cell and its ability to measure earth pressures, three forms of loads were used in its calibration, namely, hydrostatic load, dead load and earth pressures.

Hydrostatic Load.—The calibration under hydrostatic load served to demonstrate the accuracy of the cell under this type of load, and showed that the pressure required in bending the

diaphragm while breaking contact is inappreciable. The cell was placed in a vessel having a top tightly clamped down and having hose connections to a water column. Measurement of the height of this column accurately determined the pressure on the disk. A mercury gage was connected with the air line and measured the intensity of air pressure within the cell. A typical set of calibration results obtained in this way is as follows:

HYDROSTATIC PRESSURE, LB. PER SQ. IN.	AIR PRESSURE, LB. PER SQ. IN.
2.23	2.3
4.43	4.5
6.64	6.7
8.85	8.9
10.38	10.4

Readings were taken with the cell on its side as well as in a horizontal position, and no difference was observed.

Dead Load.—The dead-load calibration was accomplished by placing the cell on a platform scale and allowing one end of an I-beam to rest upon it, the other end being supported from the floor. Variation in the weight on the cell was produced by loading the I-beam with weights at various positions throughout the span. All air-pressure readings were taken on a mercury gage.

The following readings simply show the degree of accuracy which may be expected with this type of apparatus, and serve to check the area of the weighing disk which was designed to be 10 sq. in. It was found that concentrated eccentric dead loads influence the accuracy of the results and probably account for the small inconsistencies in the readings.

DEAD LOAD, LB.	READING OF MERCURY GAGE, LB. PER SQ. IN.
23	2.6
49	5.4
61	6.2
87	8.5
113	11.4
148	14.7
180.5	18.4

Earth Pressures.—The fact that the cell gave substantially correct readings under hydrostatic and dead loads was no indication that it would give true values of earth pressures, as its action depends on the breaking of electrical contact, requiring, as has been pointed out, a motion of about 0.0003 in. under high loads (50 lb. per sq. in.). Experiments made on dry quartz sand showed that a very small movement of a plunger of the same area as that of the soil-pressure disk required quite a large

TABLE I.—DEFORMATION OF MOIST SILICA SAND PRODUCED BY INCREASE IN LOAD.

Cylinder Loosely Filled.		Sand Tamped in Cylinder.	
Unit Load, lb. per sq. in.	Deformation, in.	Unit Load, lb. per sq. in.	Deformation, in.
5.0	.....	3.4	.....
5.2	0.00007	3.8	0.00010
5.4	0.00015	4.0	0.00016
5.6	0.00019	4.4	0.00023
5.8	0.00024		
9.4	.....	14.2	.....
9.6	0.00009	14.6	0.00004
9.8	0.00017	15.0	0.00007
10.0	0.00030	15.4	0.00022
20.0	.....	26.7	.....
20.2	0.00010	27.2	0.00002
20.4	0.00028	27.6	0.00007
		28.4	0.00015
28.0	.....	44.0	.....
28.2	0.00010	44.6	0.00005
28.4	0.00020	45.4	0.00010
47.0	.....	45.8	0.00022
48.0	0.00010	46.6	0.00035

increase in load. In the experiments, the sand was placed in a 6-in. cylinder, 6-in. deep, and the plunger was set on the level surface of the sand. The cylinder was then placed under load in a testing machine and the movement of the plunger for an increase in the load was measured with an Ames dial reading to 0.0001. The readings are given in Table I.

It will be noted that in general, when under high loads, a larger increase in load is required to produce a definite deformation than when under low loads. Thus, under an initial load of only 3.4 lb. per sq. in., an increase of 0.4 lb. per sq. in. was

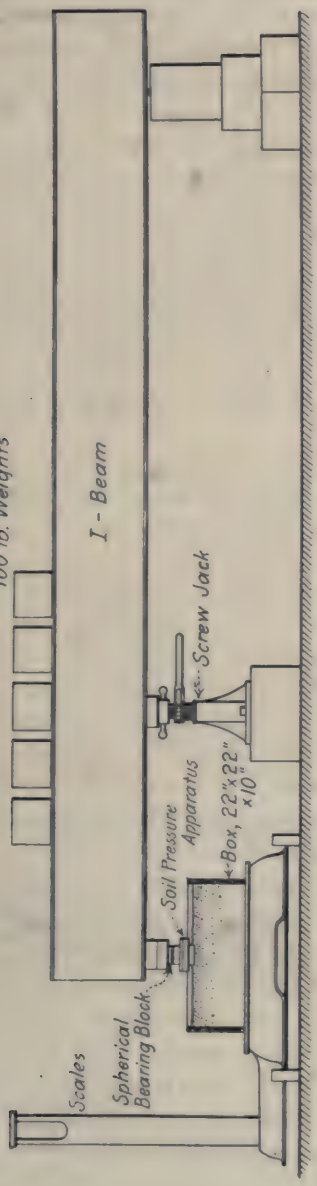

FIG. 3.—Method of Calibration of Soil Pressure Apparatus.

required to deform compacted quartz sand 0.0001 in., while under a load of 44.0 lb. per sq. in., an increase of 1.4 lb. per sq. in. was required to produce a corresponding deformation. Owing to the small size of the container, compared with the plunger, and the resulting great lateral restraint and effect of friction along the sides, it is probable that a much greater increase in load was required in this test to produce a deformation of 0.0001 in. than would ever be required in actual fills. These tests were made to gain some idea of the maximum amount of error to be expected in the cell due to its expansion against the fill. It is apparent that the error of the instrument, due to the very small motion required to break contact, should be quite small. This

TABLE II.—RESULTS OF TESTS FOR ERROR OF INSTRUMENT.

Initial Load on Disk, lb. per sq. in.	Load on Disk at Breaking of Contact, lb. per sq. in.	Load Indicated by Mercury Gage, lb. per sq. in	Error, lb. per sq. in.	Material.
10.0	10.0	10.2	0.2	Damp sand, clay.
15.0	15.0	15.6	0.6	Damp sand, clay.
20.0	20.0	21.0	1.0	Damp sand, clay.
5.3	5.3	4.7	0.5	Dry sand.
8.3	8.3	7.8	0.5	Dry sand.
11.0	11.0	11.2	0.2	Dry sand.
13.8	13.8	14.0	0.2	Dry sand.

error was further ascertained by the following method (see Fig. 3):

A box 22 in. square and 10 in. deep was placed on a large platform scales, and sand or clay was tamped in the box and leveled off. A disk 3.57 in. in diameter (10 sq. in. in area) was then placed on top of the fill and the cell was mounted thereon. Load was applied by means of weights placed on an I-beam resting on bearing blocks in contact with the cell. The amount of load imposed on the cell was regulated by means of a screw-jack, and the load was weighed on the platform scale. After the desired load had been adjusted on the disk, air pressure was admitted slowly and the scale beam carefully watched. In no case was there a perceptible rise in the beam during the increase

of air pressure within the cell, and not until after electrical contact had been broken did the beam rise, thus showing that the small elastic expansion of the cell does not appreciably vitiate the readings. The air pressure was read on a mercury gage. Table II gives typical sets of readings obtained in the above manner.

The experience gained by the authors in the actual use of this cell leads them to the opinion that the methods of trial and calibration as indicated above, are far more severe than any conditions that can possibly be met with in its designed use. It is nevertheless realized that the apparatus is capable of being improved so that it will give more precise readings, and the authors are still working toward that end. However, when the enormous variations in the condition of fills are considered, it is probable that the present error of the instrument is negligible.

It is intended to bury a number of these cells in a horizontal or vertical position within earth fills, the piping from each cell being brought to some convenient position where readings may be taken at any time. One set of indicating and controlling instruments mounted in a convenient carrying case is all that is necessary for reading any number of pressure cells. The cost of each cell when made in lots of ten should be about fifteen dollars, and the cost of one complete indicating and control apparatus without a case should not exceed forty dollars.

There are a number of uses to which the apparatus may be adapted besides that of measuring earth pressures. Thus, the pressure of concrete in forms may be determined very readily, that of ensilage in silos, the pressure of various finely divided solids in bins, and that of any material which may be slightly compressed without an appreciable increase in the pressure exerted against the instrument.

THE DEVELOPMENT OF A BOX-TESTING MACHINE AND SOME RESULTS OF TESTS.

By J. A. Newlin and T. R. C. Wilson.

SUMMARY.

A brief history of the box-testing experiments conducted by the Forest Service during the past ten years is given.

While the earlier methods used were of value in bringing out the strength of certain features or parts of a box, the results were not easily correlated with the conditions of service. This disadvantage has been largely overcome by the development of a box-testing machine of the revolving-drum type. Hazards and baffles within the drum are so arranged as to give practically all of the effects of rough handling which a box receives in service. A non-technical man witnessing the operation of this machine would see the significance of its action upon a box and could readily interpret the results obtained. The time required for these tests is considerably less than that necessary on the types of tests previously in use. The machine does not, however, duplicate those stresses which a box receives during transportation due to the other contents of the car, such as crushing, so that some of the methods previously used must necessarily supplement the tests in the revolving drum.

The essential points relative to the construction of canned goods boxes are discussed. The points brought out in these tests are:

1. The need for classification of box woods;
2. The need for much better nailing than is ordinarily practiced.
3. The superiority of cleated ends over end construction without cleats.
4. The bad effect of storing boxes in a dry heated room, after nailing up from stock in a normal air-dry condition.

THE DEVELOPMENT OF A BOX-TESTING MACHINE AND SOME RESULTS OF TESTS.

By J. A. Newlin and T. R. C. Wilson.

Introduction.

It is the purpose of this paper to report upon the work on methods of testing which the Forest Products Laboratory has done during the past year in cooperation with the Sub-Committee of Committee D-10 on Shipping Containers and the various interests represented on the sub-committee.

This work has consisted in the design, development, and operation of a machine for testing boxes of the size for carrying two dozen No. 3 cans and smaller.

Historical.

Before entering upon a description of this machine it is pertinent to review some of the previous work of the Forest Service in testing boxes and other forms of shipping containers, since the design of the present machine is to some extent the result of experience gained in the former work.

The first work of this sort consisted in tests of 171 boxes[1] at Purdue University about ten years ago.

In this series the following tests were used: Compression along an edge, Figs. 1 and 2; compression in the direction of a long diagonal or "compression cornerwise," Fig. 3; and "cornerwise impact," Fig. 4. Figs. 1, 2, and 3 are self-explanatory. In the cornerwise impact test, Fig. 4, a cross-tie spanned the distance between a stationary support and a box which was placed with its long diagonal vertical. A 250-lb. hammer was dropped from increasing heights on to this tie at the center of the span until failure of the box occurred.

The compression-along-an-edge test (see Figs. 1 and 2) can be applied in any one of three distinct ways, depending on which of the three edges meeting at a corner is chosen for the loading.

[1] Forest Service *Circular* No. 47.

Tests demonstrated that each of these ways may give entirely different results, depending upon the design and proportions of the box. In other words, a box may be very strong in one direction and very weak in another. The impact test shown in Fig. 4 was unsatisfactory because so much of the energy of the hammer was absorbed by the beam on which it dropped. The compression cornerwise test gave the most satisfactory results.

FIG. 1.—Method of Making Compression-along-an-edge Tests.

The result of this test is the same regardless of which corner receives the load. It causes the failure of the box at its weakest point and thus points out the weak features of its construction. This is a result which could be secured otherwise only by observing tests made by several methods. However, the cornerwise compression and cornerwise drop tests do not show the resistance of the box to endwise or sidewise compression.

More recently a few short series of tests on boxes,[1] barrels,

[1] Some of these are described in "Tests of Packing Boxes of Various Forms," Forest Service *Circular* No. 214. The barrel tests are described in "Tests of Wooden Barrels," U. S. Department of Agriculture *Bulletin* No. 86.

and egg-cases have been made at this laboratory. Boxes have been compressed lengthwise (see Fig. 5), edgewise, sidewise, and cornerwise, and have been dropped with their contents (see Fig. 6) so as to strike on a corner. These tests have further confirmed the conclusion that compression cornerwise or dropping on a corner are the tests which in general give a better idea of the weak features of design.

From the standpoint of a standard test to differentiate

FIG. 2.—Method of Making Compression-along-an-edge Tests.

between boxes which are and those which are not suitable for a given class of service, all these tests have the disadvantage that the results are not easily correlated with the conditions of service. A test which can be made in shorter time is also desirable.

DEVELOPMENT OF THE PRESENT MACHINE.

Before beginning work on the present machine members of the sub-committee had constructed and used a revolving cylindrical drum box-testing machine. The drum was about

4 ft. in diameter and 4 ft. long. The box to be tested was placed in the drum and the drum rotated. On the sides of the drum were inclined guides whose function was to shift the box from end to end and drop it from side to side of the drum.

This machine brought out clearly some of the defects of

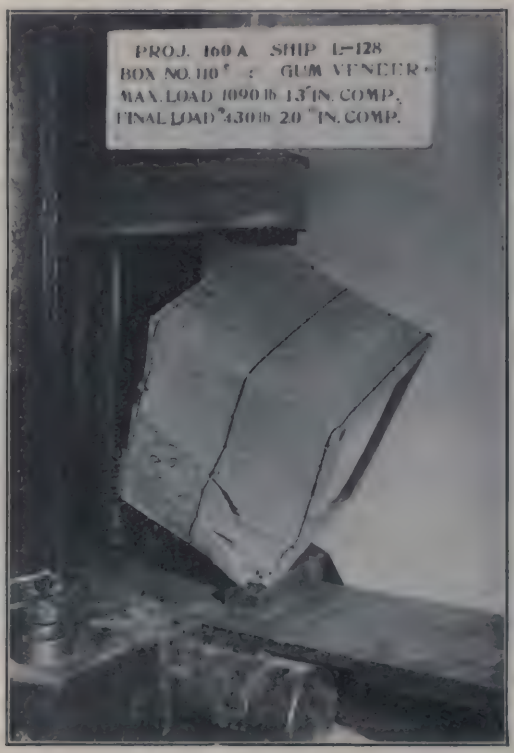

FIG. 3.—Method of Making Compression Cornerwise Tests.

various box designs but was not entirely satisfactory. The tendency was for the box to roll over and over endwise or sidewise, depending on how it was placed in the machine, thus failing to receive sufficient variety of treatment to bring out all its weak features. However, experience with this machine led to the belief that a satisfactory machine of the revolving drum type

could be produced. The design of a machine having a hexagonal drum with 3-ft. sides was begun and the drawings turned over to this laboratory. A few simple experiments made here with a box and two platforms arranged to stand at an angle of 120 deg. (the angle between the sides of a hexagon) with each other

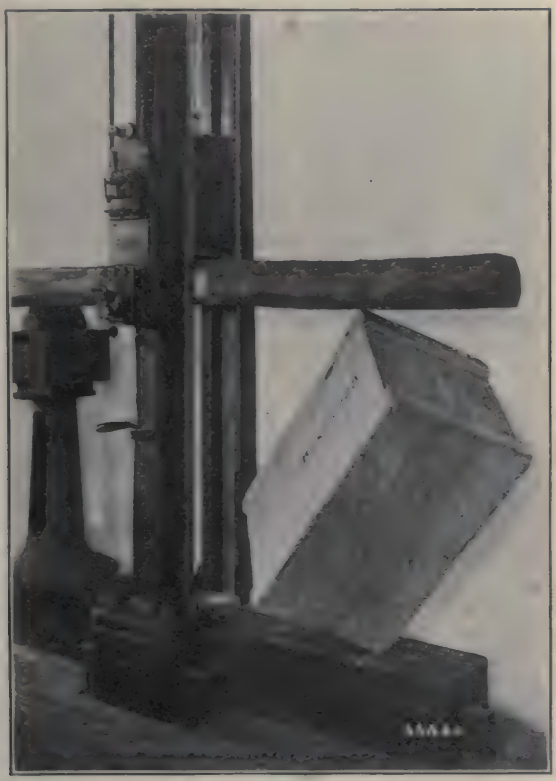

FIG. 4.—Method of Making Cornerwise Impact Tests.

encouraged the belief that a good test could be developed with a hexagonal drum machine, but showed that in order to test boxes of sizes up to that required for two dozen No. 3 cans the sides of the drum should be not less than $3\frac{1}{2}$ ft. and the inside length of the drum not less than 4 ft.

After building a drum of this size and form (see Fig. 7) with its supporting frame and rotating mechanism, there remained

to be worked out the best arrangement of internal baffles or "hazards" and the proper speed of rotation. The ideal arrangement was conceived to be such that as the box passes from side to side of the machine it would follow a regular cycle of events, including falls upon sides, top, bottom, ends, edges, and corners,

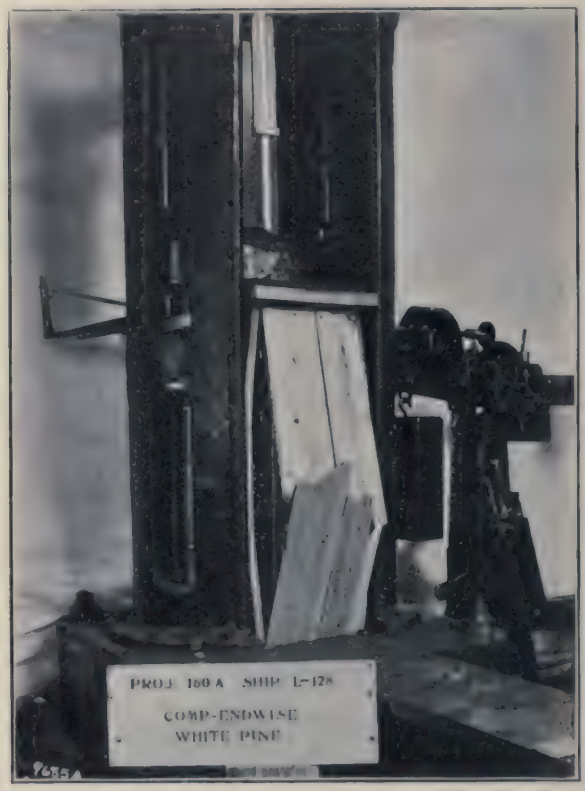

FIG. 5.—Method of Making Compression Endwise Tests.

and flatwise upon a projection similar to the corner of another box, thus simulating practically all the different shocks and stresses to which a box is subjected in transportation—with the exception of the heavy static pressure received by a box in the lower tiers of a high stack.

Scantling about 2 in. in thickness and 8 in. wide were bolted on the faces of the drum to form such hazards as it was

thought would produce the desired result. A pyramidal block was also bolted to one face. The machine was then tried out by placing a box in it, observing the behavior of the box and

FIG. 6.—Method of Making Drop Tests.

noting the number of revolutions required to render it unserviceable. The boxes used were about $12\frac{3}{4}$ in. wide by $17\frac{1}{4}$ in. long by $9\frac{5}{8}$ in. deep inside, and each contained two dozen No. 3 cans

filled with water. The weight of this size box so filled was from 60 to 65 lb.

At first the box did not slide readily on the wooden hazards and on the maple flooring with which the drum was lined, and

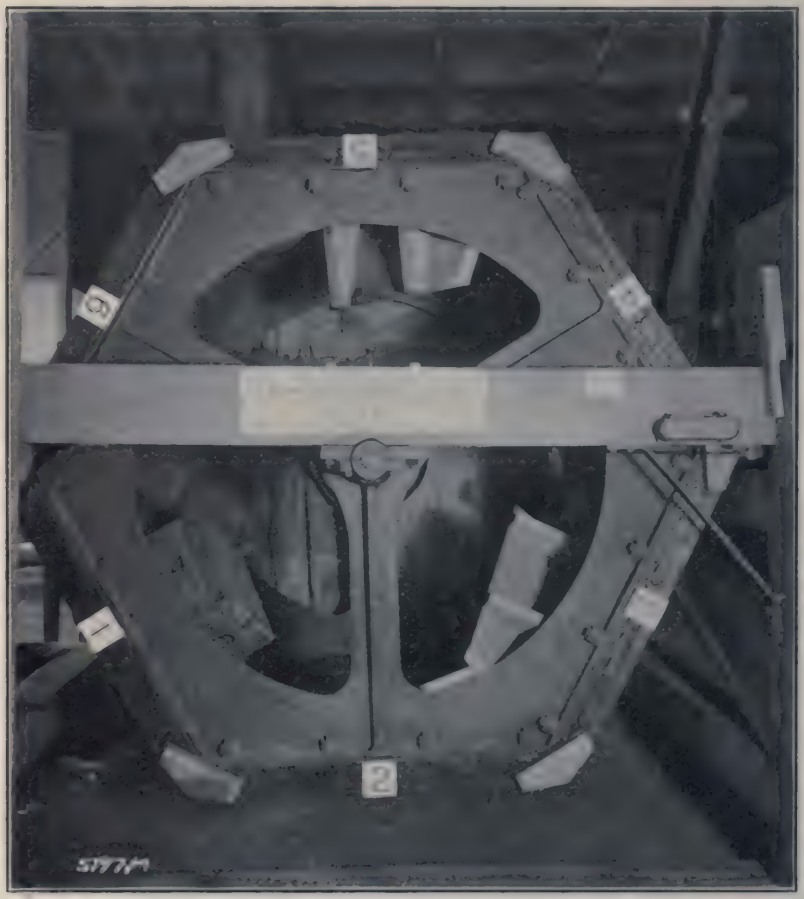

FIG. 7.—Photograph of Revolving Hexagonal Drum Box-Testing Machine.

boxes of different materials and different types did not slide with the same facility. (If all boxes were to receive the same treatment it was essential that they slide on the sides of the drum with nearly the same facility in order to come to similar

positions on the hazards.) As the machine rotated the box would frequently jump entirely over the hazards which stood up from the face of the drum only about 2 in. High baffle boards similar to those shown on sides Nos. 1, 3 and 5 (see Fig. 8) were installed to prevent this jumping. A reduction from the former speed of 4 r.p.m. to a speed of 2 r.p.m. facilitated the sliding of the boxes. Further experiments, however, demonstrated that a smoother lining was essential and the drum was faced inside with No. 11 gage soft steel. A piece of the siding had previously been taken out at each angle to permit the escape of splinters and other débris. When the lining was installed the edge of each sheet was bent over as shown in Fig. 8 to leave this space open. Scantling having their edges lined with $\frac{3}{8}$ by $2\frac{1}{2}$-in. steel were also put in. These were placed as the previous experience had indicated to be most desirable. Experiments were then continued until the arrangement of hazards shown in Fig. 8 was secured (except that the diagonal hazards had their long and short legs at approximately 90 deg. with each other). Numerous tests were made with this arrangement, running the machine at $1\frac{5}{8}$ r.p.m. Exact records of performance kept after this arrangement of hazards, together with observations of the previous tests, showed that although all boxes did not pass through exactly the same cycle of events, yet the treatment received by any box during two or more revolutions was, in point of severity, approximately the same as that received by any other box.

Some careful experiments and measurements were made to get a comparison between the behavior of fiber-board boxes and that of wooden and wirebound boxes. The friction of fiber board on the steel lining is greater than that of wooden and wirebound boxes. (The friction of wooden and wirebound boxes is approximately the same and the friction of solid fiber does not differ appreciably from that of corrugated fiber.) The greater friction of the fiber board causes boxes of this material, when resting flat on the side of the machine, to be carried higher before beginning to slide, and then to slide with approximately the same velocity upon the surface with a greater slope. The effects of this retardation are that the fiber box is carried higher before it comes against the guiding hazards (of sides Nos. 1, 3 and 5) and although the side of the machine is at a

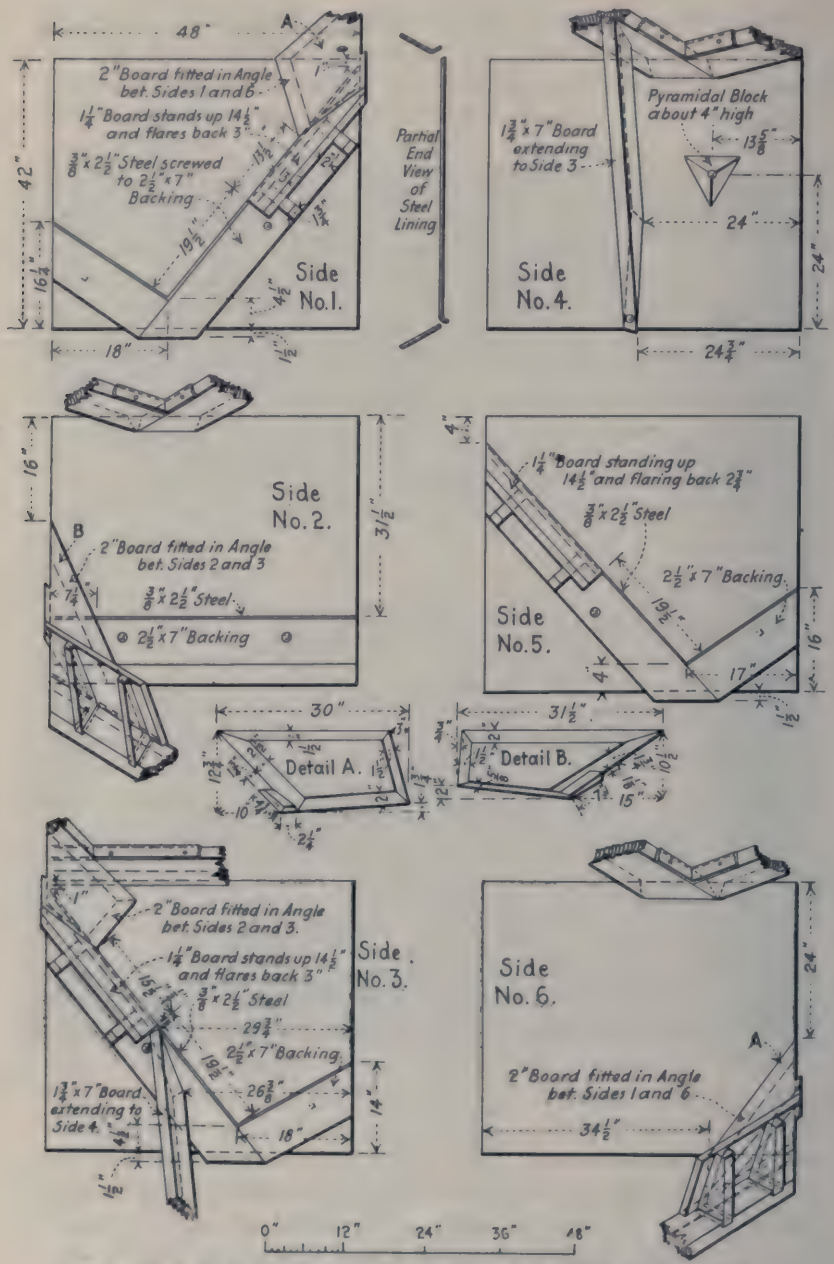

FIG. 8.—Details of Arrangement of Hazards in Revolving Hexagonal Drum Box-Testing Machine.

greater angle, both before and at the time the box reaches the guiding hazard, the impact against this hazard is very nearly the same as in the case of the wooden box. All boxes slide with sufficient readiness to come into position against these hazards some little time before falling to the next side of the machine. However, the deformation and battering which the fiber box receives in the earlier part of the test permits its center of gravity to advance (in the direction in which the box is moving with respect to the side of the machine) beyond its normal position (that is, the position it would have were the box perfectly rigid or nearly so) upon or soon after striking the hazard, with the result that the fiber box falls from the hazard earlier, and consequently from a lesser height than the more rigid wooden box.

As previously mentioned the diagonal hazards on sides Nos. 1, 3, and 5 were first made with their long and short legs at approximately right angles to each other. It was found that with this arrangement a box would frequently become slightly wedged in one of these angles and would be carried abnormally high before falling to the next side, thus suffering an unusually severe drop. To obviate this difficulty these angles were increased to about 96½ deg. Since this change the variation in the drops from side No. 1 to side No. 2 is not appreciable— likewise from No. 3 to No. 4 and from No. 5 to No. 6. The horizontal hazard on side No. 2 was at first 15 in. from the edge of the side. This distance was decreased to 10½ in. to permit a box when falling from side No. 1 to strike flat on side No. 2 instead of striking across the edge of the hazard as frequently happened before this change was made.

During the development of the machine a large number of tests was made on boxes of various kinds. On 61 of these, and on about 200 of the tests made since the last change in the machine, records were kept to show the position of the box on each side of the machine during 26 complete revolutions or until failure of the box. The method of keeping these records is as follows: The top of the box is numbered 1, the bottom 3, the sides 2 and 4, and the ends 5 and 6, as shown partially in Fig. 9. Points around the perimeter of each face of the box are numbered 1 to 8, as is also shown in Fig. 9. The position of the box on any side of the machine is indicated by two figures; the first shows

which face of the box is up, the second shows which point in the perimeter of this face is up. For instance, in Figs. 7 and 9 face No. 1 is up and point No. 8 of that face is up; this position is designated 18 or, one-eight. The two sides of a sample record sheet are shown herewith, as Tables I and II respectively. In these records *A* indicates the first revolution of the machine, *B* the second, etc. In the sample record *A* appears in column

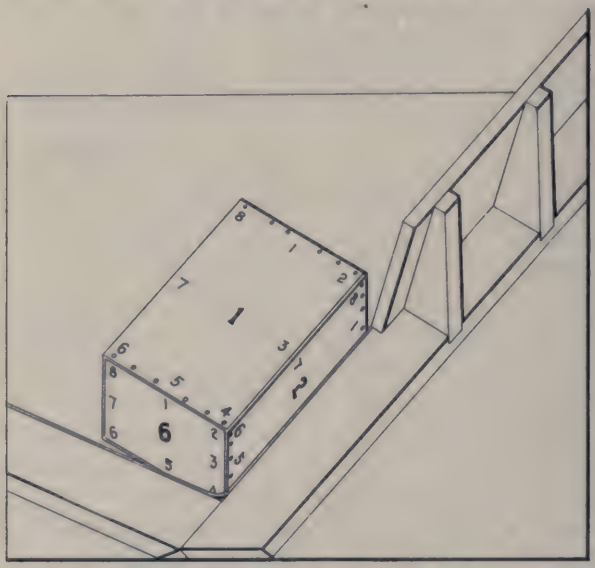

FIG. 9.—Position of Box on Side No. 1 at Beginning of Test, showing Numbering of Sides of Box and of Points around the Perimeter of the Sides.

No. 1 (side of machine) opposite "Position of Box" 18; in column No. 2 opposite position of box No. 37, etc.; which means that the test was started with the box in position 18 (as shown in Fig. 9) on side No. 1, fell over to position 37 on side No. 2, thence to position 16 on side No. 3, thence to position 21 on side No. 4, etc.

A column is provided for notes taken during the test. The other side of the record sheet, shown in Table II, provides for the description of the box made before test.

Numerous experiments were made to determine which

position of the box at the beginning of the test was most desirable. The conclusion reached is that the position shown in Figs. 7 and 9 is the one which will result in the greatest uniformity of treatment as between different types of boxes.

The last change in the hazards was made on December 23, 1915. Since then, tests have been made on about 185 nailed and about 75 wirebound boxes. No further changes of hazards are believed desirable except to replace the pyramidal block on side No. 4 by one of more permanent material—probably cast iron—held in place by a lag bolt inserted from outside the machine.

The machine as now arranged is quite efficient in exposing the weak features in the construction of any wooden box and in furnishing data on which to base specifications.

Limitations of the Machine.—This machine is, of course, adapted only for boxes of the size for two dozen No. 3 cans—and smaller. Its operation is intended to simulate the roughest handling which a box should receive in service and to measure comparative resistance to such usage. It does not, of course, offer a measure of the resistance of a box to heavy static pressure.

Operation of Machine.—The best results have been obtained by beginning the test with the box in the position shown in Fig. 9 and rotating the machine at a speed of $1\frac{5}{6}$ r.p.m. This speed permits the box to come to rest upon the hazard. It also allows notes to be made of the kind and sequence of the failures.

TESTS OF NAILED BOXES.

The 185 nailed boxes tested consisted of 148 for two dozen No. 3 cans and 37 for two dozen No. 2 cans, including: Several species of lumber as follows: white pine, southern yellow pine, Sitka spruce, western yellow pine, yellow birch, aspen ("northern poplar" or "popple"), yellow poplar (tulip-tree), and red gum; end construction as follows: Single piece, 2-piece cleated, 2-piece with corrugated fasteners, and dovetailed; thickness of ends—$\frac{1}{2}$, $\frac{9}{16}$, $\frac{5}{8}$, and $\frac{7}{8}$ in.; sides, tops, and bottoms of $\frac{1}{4}$, $\frac{5}{16}$, and $\frac{3}{8}$ in. sawed lumber and $\frac{3}{16}$, $\frac{7}{32}$, $\frac{1}{4}$, $\frac{9}{32}$ and $\frac{5}{16}$ in. rotary cut veneer; nailed with 4, 5, 6, and 7d c.c. nails—4 to 9 nails per nailing edge.

TABLE I.—TEST OF BOX IN TUMBLING MACHINE.

Proj. 207. Ship. No. 425. Box No. 67.
Laboratory No. 133,016. Date May 12, 1916.
Counter: Start 928,600. Stop 928,682. Difference 82.

Position of Box.	Side of Machine.						Notes.
	1	2	3	4	5	6	Counter.
11		*J*		*J*		*J*	
2					*H*		
3						*L*	
4	*N*					*FK*	
5				*F*			
6			*ABN*		*N*		
7						*CD*	
8	*AB*						
21				*A*			
2		*CI*					
3			*FK*		*M*	*I*	
4	*LM*				*L*		
5		*F*		*C*			
6					*G*		
7				*OE*			
8		*OE*					
31		*N*		*N*			
2					*F*		
3			*L*		*K*	*H*	
4	*JK*						
5				*H*			
6		*G*	*J*		*J*		
7		*A*	*OE*			*A*	
8	*F*				*E*		
41				*BG*			
2	*CI*					*G*	
3		*K*		*KLM*	*I*		
4		*LM*				*M*	
5							
6							
7		*B*					
8	*DE*				*CD*	*E*	

TABLE I.—TEST OF BOX IN TUMBLING MACHINE—*Continued.*

Position of Box.	Side of Machine.						Notes.
	1	2	3	4	5	6	Counter.
51					*A*		
2							
3							
4			*I*				
5							
6					*B*		
7	*H*		*H*				
8			*C*				
61							
2							
3							
4							
5							
6	*G*			*I*			
7		*H*				*B*	
8			*C*				

TABLE II.—DESCRIPTION OF BOX.

Style, Nailed.
Size (Inside), 17 by 12¾ by 9¾ in.
Weight; Box, 8¾ lb.; Box and contents 64½ lb.
Kind and Thickness of Material, Southern Yellow Pine.
Sides, ⅜-in. 2pc. matched. Top and Bottom ¾-in. 3pc. matched.
Ends, ⅜-in. 2pc. matched.
Cleats, 1⅛ by ⅞ in.
Size of Wires,—
Number of Wires, None.
Size, Kind and Spacing of Nails, 6 5d. cc. box nails to each nailing edge.
Cleats to ends, 6 3d cc. box.
Nailed on May 4, except top. Top nailed on May 11.

TEST OF WIREBOUND BOXES.

About 75 wirebound boxes were tested. These included boxes for No. 3 and No. 2 cans bound with No. 16 and No. 15 wires; stapled with 14, 18, 20, and 24 staples per wire; with stapled, nailed and loose ends.

DISCUSSION OF ESSENTIAL FEATURES.

It is now in order to discuss the more essential points brought out by the tests.

Classification of Box Woods.—The following classification of box woods is the result not only of the box tests described herein, but of tests made by this laboratory on the weight, strength, hardness, shock-resisting ability, resistance to the withdrawal of nails, etc., of the woods in question.

This classification recognizes that the proper size of nail is dependent not only on the thickness of the material but also upon the resistance of the wood to nail-withdrawal.

GROUP NO. 1.

Alpine fir
Aspen
Balsam fir
Basswood
Buckeye
Butternut
Cedar
Chestnut
Cottonwood
Cucumber
Cypress
Magnolia
Noble fir
Redwood
Spruce
Sugar pine
Western yellow pine
White fir
White pine
Willow
Yellow poplar

GROUP NO. 2.

Douglas fir
Hemlock
Larch
Southern yellow pine

GROUP NO. 3.

Ash
Beech
Birch
Black gum
Elm
Hackberry
Hickory
Maple
Oak
Red gum
Sycamore
Tupelo

Group No. 1 includes the woods which are softer and less resistant to nail-withdrawal and consequently require larger nails.

Group No. 3 includes the woods which are harder and more

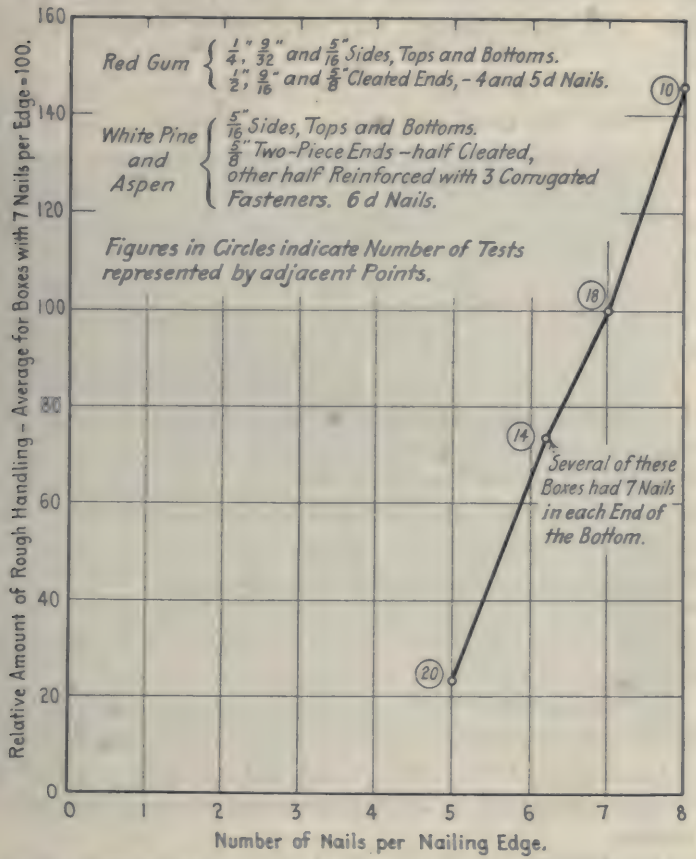

FIG. 10.—Tests of Nailed Boxes for Two Dozen No. 3 Cans; Relation of Number of Nails to Amount of Rough Handling Required to cause Loss of Contents.

resistant to nail-withdrawal and consequently require smaller nails, and slightly less thickness of material in the box ends.

Group No. 2 is composed of woods of intermediate quality. These are the harder coniferous woods. The woods in this group are also much more subject to shakes, to heavy checking

in drying, and to splitting in nailing or under rough handling than are woods of the other groups. They are considerably more variable in strength and resistance to the withdrawal of nails than are the woods included in the other two groups. For this reason it is recommended that ends made of these woods be cleated.

Nailing.—The most striking development in the tests of nailed boxes was the demonstration of the inadequacy of nailing as ordinarily practiced (which was shown in the case of many of the boxes sent nailed up by the manufacturer), and of the very great improvement which can be accomplished by proper nailing. Also in many cases proper nailing will make possible a reduction in the amount of lumber used and without any decrease in the value of the box. The influence of variations in nailing upon the strength of the box is shown in Fig. 10.

Cleated Ends.—Some of the earlier tests at this laboratory had shown the relative inefficiency of ends without cleats (either single-piece or two-piece ends). The present tests give further evidence of this. White pine, red gum, and aspen boxes with $\frac{5}{8}$-in. two-piece ends, matched and reinforced with 3 corrugated fasteners averaged only two-thirds the resistance of boxes which were of the same identical construction except that the corrugated fasteners were replaced by two $\frac{3}{8}$ by $1\frac{1}{8}$-in. cleats. Failure in the uncleated boxes was due to splitting of the ends and did not occur at the joint.

In similar tests yellow-pine boxes with uncleated ends were only one-half as resistant as when cleated ends were used. This does not include some boxes which did not hold together until they could be placed in the machine, and whose failure was due to shakes.

Over-Driving of Nails.—A few cases were observed in which nails were quite seriously over-driven. Fig. 11 illustrates an instance of this. As may be seen the nail-head is driven through about one-third of the thickness of the board, which in this case was $\frac{1}{4}$ in. It is very evident that the wood fibers are distorted and damaged throughout practically the entire thickness of the board, and over a considerable area around the nail head. It was to be expected, and observation of the tests showed,

that the holding ability of such over-driven nails was very seriously impaired. The board is quite easily separated from the nail either by pulling the nail head through the board or by pulling the board endwise from the nail. Data are not available to give an exact measure of the decrease in the resistance of the box. However, it is easily seen that very great damage is likely to result from the practice of over-driving. Care should be

FIG. 11.—Showing an Over-driven Nail (× 4). Note Crushed, Broken, and Distorted Wood Fibers around and under the Head of the Nail.

taken to drive nails only sufficiently deep to bring their heads flush with the surface of the box.

Effect of Storage of Boxes after Nailing.—It was also observed in these tests that there was frequently considerable loss of resistance brought about by keeping boxes (after they were nailed up) in the laboratory where the air was quite warm and dry.

This effect is manifested under two different conditions:

1. If the material contains sufficient moisture that storage

results in further drying, shrinkage takes place. This causes a lateral or sidewise pull on the nails (even on those holding sides to ends since the ends because of their greater thickness do not shrink so quickly as do the sides; but more particularly those holding tops and bottoms to ends since this shrinkage of the ends in the direction of their length is practically *nil*) and if the increased drying is sufficient splitting at the nails results. This side pull on the nails loosens them slightly and causes them to be more easily pulled while the splitting at the nails enables the boards to be pulled from them more readily.

2. A reduction of the holding power of cement-coated nails has been observed even when no change in the dryness of the wood into which they are driven takes place.

There are no doubt many things which influence these phenomena and further investigation will be required before quantitative statements can be made in regard to them. Tests have demonstrated, however, that the reduction due to these causes is sufficient reason that the causes should be eliminated as far as possible. For this reason it is recommended that boxes should not be nailed up of material containing more than 15 per cent moisture and that they should not be stored in a dry heated room after nailing.

Wirebound Boxes.—The tests on wirebound boxes demonstrated that at least 20 staples per wire are required to produce a box capable of properly protecting its contents. With fewer staples the force acting on the individual staples is sufficient to cause the veneer to be pulled from under the staple quite readily. When 20 staples per wire were used failure was much less frequently caused by pulling of veneer from under staples than when a smaller number of staples was used.

Boxes for two dozen No. 3 cans made of $\frac{1}{8}$-in. veneer and with 20 staples per wire (six at top, six at bottom, and four at each side) averaged 30 per cent more resistance than boxes with 14 staples per wire (4 at top, 4 at bottom, and 3 at each side). Boxes with 24 staples per wire averaged 25 per cent more resistance than those with 20 staples. Fig. 12 shows the relation of number of staples to resistance to rough handling.

It was also shown that staples connecting ends to cleats were very inefficient unless driven at a considerable angle with

the grain of the end. Either broad-headed nails or properly driven staples are effective in holding ends to cleats.

The tests on wirebound boxes showed that considerable care was necessary in the manipulation of thin veneers. Veneer of $\frac{5}{32}$ and $\frac{1}{8}$ in. absorbs moisture very readily when exposed to a damp atmosphere and loses it as readily when brought into a dry one. Because of this sensitiveness it is desirable that boxes

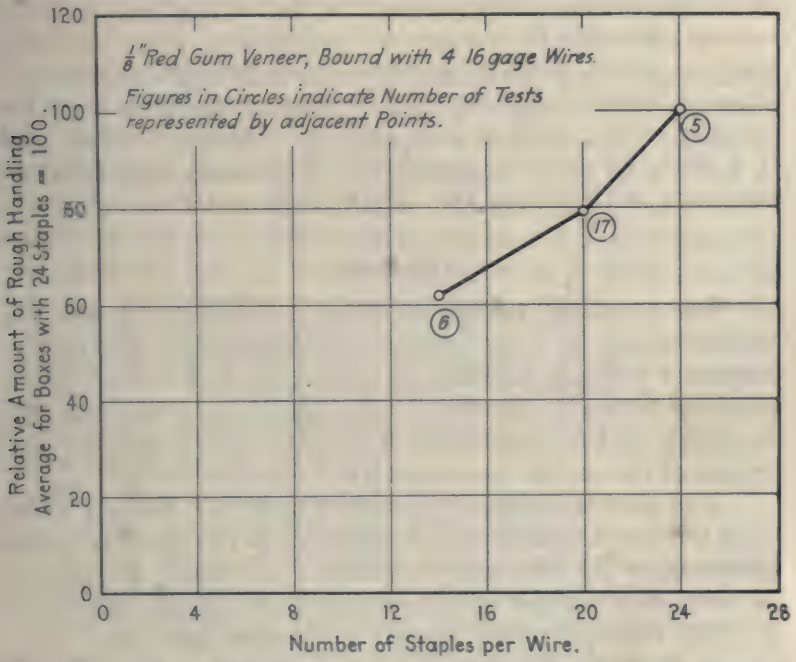

FIG. 12.—Tests of Wirebound Boxes for Two Dozen No. 3 Cans; Relation of Number of Staples to Amount of Rough Handling Required to cause Loss of Contents.

made of such material be exposed as little as possible to changing humidities in order to avoid shrinkage and swelling, which splits and loosens the veneer from around the staples. The drying which sometimes takes place in a dry heated room is also sufficient to cause a marked brashness or loss of "life" in thin stock.

It has further been shown by Forest Service tests that there is also a loss of inherent shock-resistance when wood is dried

to very low moisture content; that is, if lumber is dried down to say 5 per cent moisture and then exposed to such climatic conditions that it reabsorbs to 15 per cent moisture it is less resistant to shock than lumber which has not been dried below 15 per cent. This effect is augmented if the lumber is dried by excessively high temperatures or low humidities. Of course, the climatic conditions to which boxes and box materials must be exposed are not subject to control; but drying to a moisture content lower than the average air-dry condition, 10 to 15 per cent moisture, serves no useful purpose, except perhaps to lessen the shipping weight (and this will not be accomplished unless the shipping weight is taken soon after the material is dried), and it is likely to result in considerable damage by increasing the brashness, or decreasing the "life," of the material.

RÉSUMÉ.

The revolving hexagonal drum box-testing machine described herein is quite efficient in determining the relative resistance to rough handling of various types of wooden boxes, exposing the weak features of their design and furnishing data on which to base specifications for satisfactory shipping containers of the size for two dozen No. 3 cans and smaller.

The number of drops required to cause the contents to fall from the box, together with observations of the tests and of the condition of the box, and its contents, at various stages, may be taken as a measure of its resistance to rough handling.

The principal points brought out in these tests are:

1. Need for classification of box woods;
2. Need for much better nailing than is ordinarily practiced;
3. The superiority of cleated ends over end construction without cleats;
4. The bad effect of storing boxes in a dry heated room after nailing up from stock in a normal air-dry condition.

METHOD FOR TESTING THE DURABILITY OF PIPE UNDER CORROSION.

By F. N. Speller.

All will agree that a service test, or at least a test under service conditions, is the only reliable method for determining the relative durability of iron and steel products. However, to be of any value it must be possible to carry such a test to conclusion in a reasonable time. The so-called "acid test" quickly attracted attention, but as this proved to be misleading the effect has been to discount any attempt to accelerate corrosion. Nevertheless, there are certain conditions commonly found in service where pipe is subject to aerated water at a temperature of 140 to 180° F. under pressure, where corrosion is very severe. The method of testing here referred to works under such conditions, which are more or less common to all water and steam-pipe service.

The main factors controlling corrosion in pipe are:

1. The amount of free oxygen in solution in the water;

2. The volume of flow, which is an important factor, mainly on account of the greater amount of oxygen available; and

3. Temperature. Corrosion increases with the temperature, reaching a maximum somewhere between 160 and 180° F.

These conditions are reproduced in the apparatus shown in Fig. 1. This consists of a tank or reservoir open at the top, which may be filled with a supply of any kind of water selected for the test. This water is maintained at any desired temperature by means of a small gas heater *B* fitted with a thermostat, or a steam coil may be inserted if more convenient. An ejector is provided at *C* and operated by air under six to eight pounds pressure. The air should first be passed through a mass of excelsior or similar material to remove oil. (*A*, Fig. 1.) The heated water is thus aerated, forced up through the coil of pipe and discharged continuously into the top of the tank. The coil

consists of alternate lengths of the materials under test. A glass tube may be inserted in the coil if it is desired to observe the flow. By keeping up a continuous flow in this way at a temperature of 160 to 180° F. the relative tendency of pipe to corrode may be determined in three to four months. The amount of corrosion found in that time with this apparatus will be found equal to about two years under the same conditions in actual service.

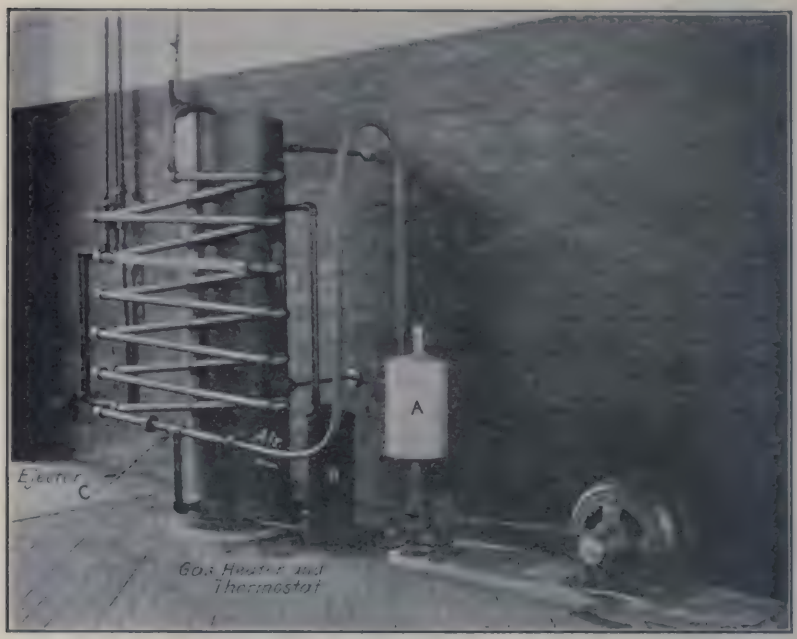

FIG. 1.—Apparatus for Corrosion Testing, using Aerated Hot Water.

The acceleration of corrosion in this apparatus is principally due to the continuous flow and the greater amount of oxygen and carbonic acid which is thereby brought into contact with the interior surface of the pipe. The details of this machine may be altered according to local conditions. If the water is not fresh and almost pure, the supply should be renewed from time to time.

The amount of corrosion in both wrought-iron and steel hot-water-supply pipe, as measured by the depth of pitting, will frequently amount to about 0.1 in. in two years. We have found with this apparatus nearly this amount of corrosion in pipe of this kind in three months, with the water circulating at the rate of one to two gallons per minute. The relative depth of pitting is apparently the same in each case.

The writer has used this scheme for two years in research work on the improvement of pipe and pipe coatings, with much saving of time compared with similar tests on pipe in actual use.

DISCUSSION.

Mr. Schuhmann.

MR. GEORGE SCHUHMANN.—When the discussion about the relative corrosion of wrought iron and steel started about ten years ago, the claim that steel had a greater resistance than wrought iron was based exclusively on accelerated corrosion tests, principally on the so-called "acid test," that is, by the immersion of wrought-iron and steel samples in solutions of strong acids and then comparing the loss of weight in a few days or hours. We pointed out then that the behavior of these metals in actual service contradicted the accelerated corrosion tests.[1] This contention has been fully verified by subsequent experiences and is now admitted by all metallurgists who have kept posted on this subject, and even Mr. Speller states in his paper that the acid test is misleading.

We have also pointed out that it is not only the acid test that is misleading, but also all accelerated corrosion tests or extraordinary service tests where the corrosion is so rapid as to destroy the metal in a short time. Combustion is oxidation; so is corrosion, as well as the slow decay or rotting of wood. The only difference is in their respective intensities; yet we cannot predict the resistance of objects to the latter two by subjecting them to accelerated oxidation or combustion or exposing them to the action of acids any more than we can foretell the resistance of wood against slow decay or rotting by burning samples of the wood in a hot furnace. By impregnating wood with coal tar or other similar oily substances, we can greatly increase its resistance against rotting, while the very presence of these preserving substances will accelerate the combustion of the wood; and therefore this accelerated test would give results indicating the exact opposite to what the same wood would do in the slow decay of actual use. The cinder or slag with which wrought iron is impregnated plays a similar rôle in regards to resistance against slow corrosion, as has been most clearly demonstrated by Mr. R. C. McBride, of Youngstown, O., by his corrosion tests, which

[1] *Proceedings*, Am. Soc. Test. Mats., Vol. VIII, p. 267 (1908).

were published in the technical journals about five years ago.[1] In conclusion he states: "The greater susceptibility of iron to accelerated corrosion is due to the fact that the diluted acid employed readily attacks this protective coating, and, by opening up the foliated structure in this way, multiplies the surface exposed to corrosive action. It is well known that slag resists, almost to the point of immunity, the corrosive agents of actual service. . . . The slag content, while a protection in actual service, entirely vitiates the accelerated test as applied to such materials." Mr. Schuhmann.

The apparatus described by Mr. Speller accelerates corrosion by the introduction of large quantities of oxygen, which is blown in by a strong air stream, and while such an apparatus will no doubt give information as to the comparative rate of corrosion of different pipes under conditions identical as regards temperature and excess of oxygen, the results are no criterion as to what the same pipes will do when exposed to the slow corrosive influences to which most pipe is subjected in ordinary service. While the introduction of oxygen does not cause the corrosion to be as rapid as by using acid, yet its object as well as its result is acceleration, and it is therefore as unreliable as the acid test.

MR. F. N. SPELLER.—Lest there should be any wrong impression received from Mr. Schuhmann's remarks, I should like to point out again, and emphasize the fact, that this is not an accelerated test in any sense of the word. All the conditions are the same as those commonly found in service in water and heating lines. Corrosion slows down considerably in a water line when the water lies dead, so that the rate of corrosion probably varies from a minimum to a maximum many times throughout the day. In the apparatus which we have used circulation is provided 24 hours a day and the water is kept thoroughly aerated during this time so the action is continuous instead of intermittent. For this reason the time required to produce a measurable amount of corrosion is greatly reduced. Mr. Speller.

This is not a substitute for service tests, for when it is possible to put various grades of pipe alternately in the same line in service, it is probably better to do so, provided the condi-

[1] *Engineering Record*, May 20, 1911.

Mr. Speller. tions are such that the test will be completed inside of a reasonable length of time. We cannot afford to wait a life time, however, to determine the facts on an important question of this kind, nor is it necessary. We have found that the results obtained by the use of a testing system such as that described in this paper are the same as similar tests covering a period of two or three years in actual service in the same water. Moreover, the corrosion apparatus enables us to make a test in one place under proper supervision using any kind of water that may be desired. In a railroad laboratory, for instance, the question might come up as to the effect of certain waters from various divisions on piping and tubing. This could be determined with sufficient accuracy, I believe, by means of an apparatus of the kind described in the course of six months or so. It is desirable to let the test run until the pipe is corroded at least half way through and then open up the samples in a planer, scrape clean or preferably clean with 10-per-cent ammonium-citrate solution, and measure the depth of pitting with a depth micrometer.

PRACTICAL METHODS FOR TESTING REFRACTORY FIRE BRICK.

By C. E. Nesbitt and M. L. Bell.

SUMMARY.

The necessity for a satisfactory and standard method of testing refractory fire brick has long been felt. To be of the greatest benefit tests should be such that they may be readily made on the finished brick. The purpose of this investigation was to develop tests that would imitate service conditions, and give information from which the quality of the brick could be determined.

The influence on the various tests of the moisture content in the pug is brought out strongly in Table I. In common practice more water is used in the pug than is necessary to give the best results. The comparison between a high-pressure, low-moisture brick and a brick made by hand by the regular methods, as given in Table II, shows a great difference in the test results.

The methods of testing were developed after trying many different kinds of tests. Some tests were discarded, portions of others were used, and then combined into a method (appended to the paper) which has proved by experiment to be valuable in showing a particular quality of a brick. The methods described are primarily intended for testing brick about an iron and steel plant, but it is believed that they can be easily modified to fit the requirements of brick in other industries.

PRACTICAL METHODS FOR TESTING REFRACTORY FIRE BRICK.

By C. E. Nesbitt and M. L. Bell.

The use of refractory material in metallurgical operations is one which, as a rule, is given but very general consideration. The selection of a suitable refractory calls for a study, first, of the temperatures involved, and secondly, of the nature of the hot material and resulting slag to which the refractory will be exposed.

Refractories are generally used in form of bricks or blocks which are molded from materials in the plastic condition, then burned. The resulting product is a solid of sufficient mechanical strength to permit its use for various forms of construction.

The two most common raw materials for making refractory bricks are clay and a variety of silica called ganister. Other raw materials are gradually finding wider and more general use, but these two continue to be the important ones.

Clay and silica rock suitable for bricks are widely distributed, but nature has very largely determined their physical properties. Each of these materials has varying physical properties which may be taken advantage of and incorporated into the finished bricks, the quality of which is largely dependent on the methods employed in their manufacture.

In its simplest terms a furnace or heating chamber consists essentially of a shell of brick work sometimes bound together with a suitable frame work of iron. Owing to various physical, chemical and mechanical operations which take place in the furnace, the refractory must meet certain demands to give a satisfactory life. No brick can be expected to be best in all physical properties, and it becomes necessary to select the brick which is best suited to withstand that particular condition imposed by the furnace. It may be that the brick will have to withstand the abrasive action of heavy, hard, materials, the sudden fluctuation of temperature, the corrosive action of slag, or the action of load while at high temperatures. These and

similar points should be considered, their relative importance determined, and the brick chosen should be the one best suited to resist that particular condition.

In the iron and steel industry the blast furnace is one of the most important types of furnaces. In simple terms it is a hollow steel cylinder, approximately 100 ft. high and some 28 to 30 ft. in diameter, lined with fire brick. In a furnace of 500-tons capacity about 1900 tons of ore, limestone, and coke are used every 24 hours. These materials, often wet or frozen, are fed in at the top and fall on the materials already in the furnace or roll over against the walls. Since this material must pass slowly through the furnace the wear of the lining is extremely heavy. The temperature at the top of a blast furnace is low, but as the charge descends the temperature increases until the fusion zone is reached. In this zone the hard material composing the charge becomes a pasty mass which ultimately is melted, and the brick work then has to withstand molten iron and slag rather than abrasion or impact.

No blast furnace is complete without its battery of stoves, in which no metallurgical operations take place. The object of the stove is to absorb heat rapidly and then as quickly give it up. Here a brick must withstand sudden thermal changes, abrasion, slagging, due to the dust carried by the burning gas, and possess good heat-absorbing qualities.

In one of the processes of converting pig iron into steel a basic open-hearth furnace is used. This furnace is built up largely of silica brick, although the hearth is made up of basic materials because of the basic character of the slag, while the regenerators are of fire brick. Silica bricks are very sensitive to thermal changes, but owing to their infusibility and their ability to withstand heavy loads when highly heated, they are used almost without exception in open-hearth practice. Any variation in manufacture of material which will tend to decrease their sensitiveness to heat changes, without sacrifice to their other good qualities, will directly increase the life of the furnace.

In soaking pits, heating furnaces, annealing furnaces, mixers, ladles, etc., the temperatures are not as high as in the open-hearth furnace; the brick work here has mainly to withstand the action of the molten metal, iron scale, or slag.

A general review of the conditions imposed on fire brick as used about iron and steel plants has pointed out two general conclusions: (1) the life of a furnace is determined by the life of its brick work, and (2) different furnaces demand different physical properties in the brick used.

DESCRIPTION OF TESTS.

A great many investigators have attacked the problem of refractories, the work having been done mainly from the standpoint of the inherent physical properties of the raw materials.

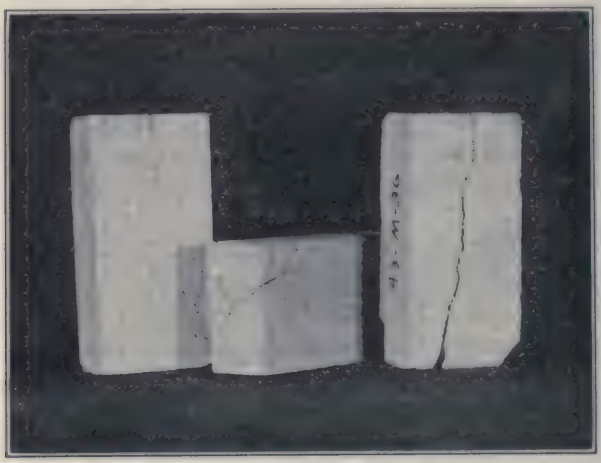

FIG. 1.—Impact Tests, Illustrating Characteristic Fractures.

The results are of high technical excellence and have been of value. In the majority of cases, however, the tests have not been applicable to brick as received by the consumer. He could not go out to his stock, select several bricks and quickly determine their value for his particular purpose.

With this idea in mind each of the various conditions where fire brick are used was studied to determine the cause of failure. The next step was to imitate actual conditions of service in the laboratory. This meant that tests must be employed which were very severe, producing in a short time what in actual service might require months or even years to accomplish. To test brick

FIG. 2.—Abrasion Tests, Showing Difference in Depths of Cuts in Upper Brick.

FIG. 3.—Slag Penetration and Abrasion on Silica Brick—Portion of an 18-in. Bulkhead.

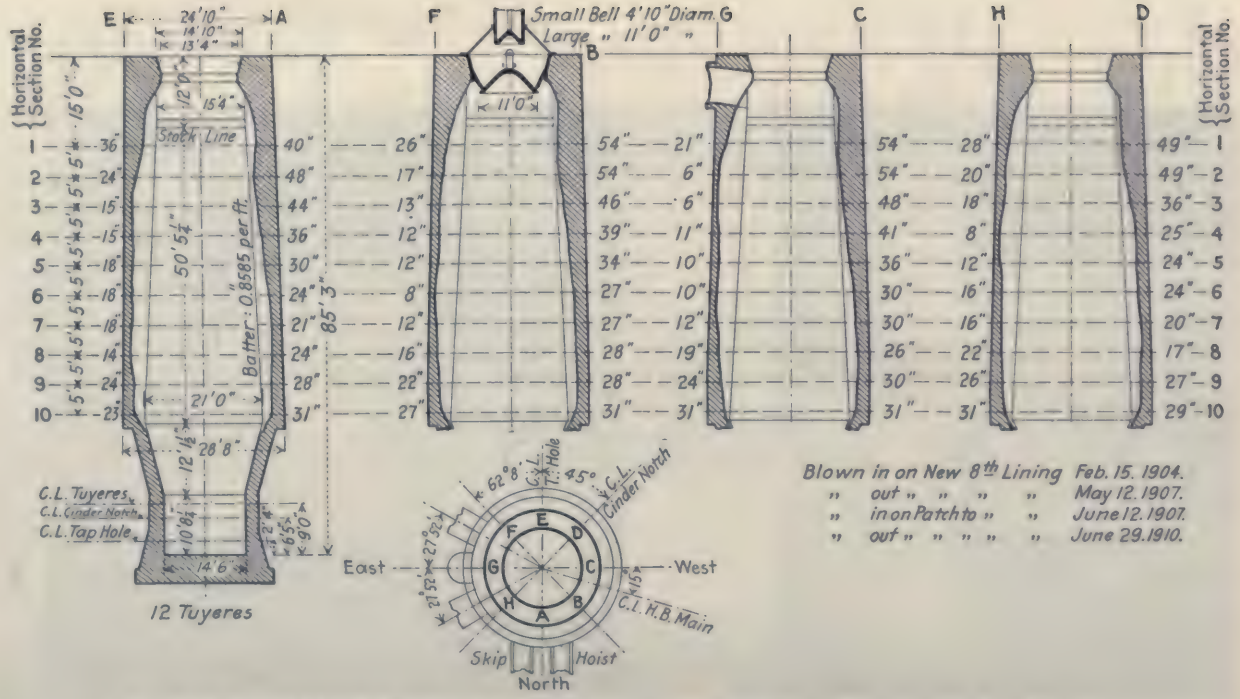

FIG. 4.—Vertical Lines of Erosion on a Blast Furnace after Six Years' Service—Uniform Wearing.

NOTE.—The letters *E*, *A*, etc., represent vertical sections taken on the diameters shown in the plan; the figures 1, 2, 3, etc., shown at the left and right, designate the horizontal sections, which are shown in Fig. 5. The figures on the outside of the vertical sections represent the thickness of the brick lining remaining at these points.

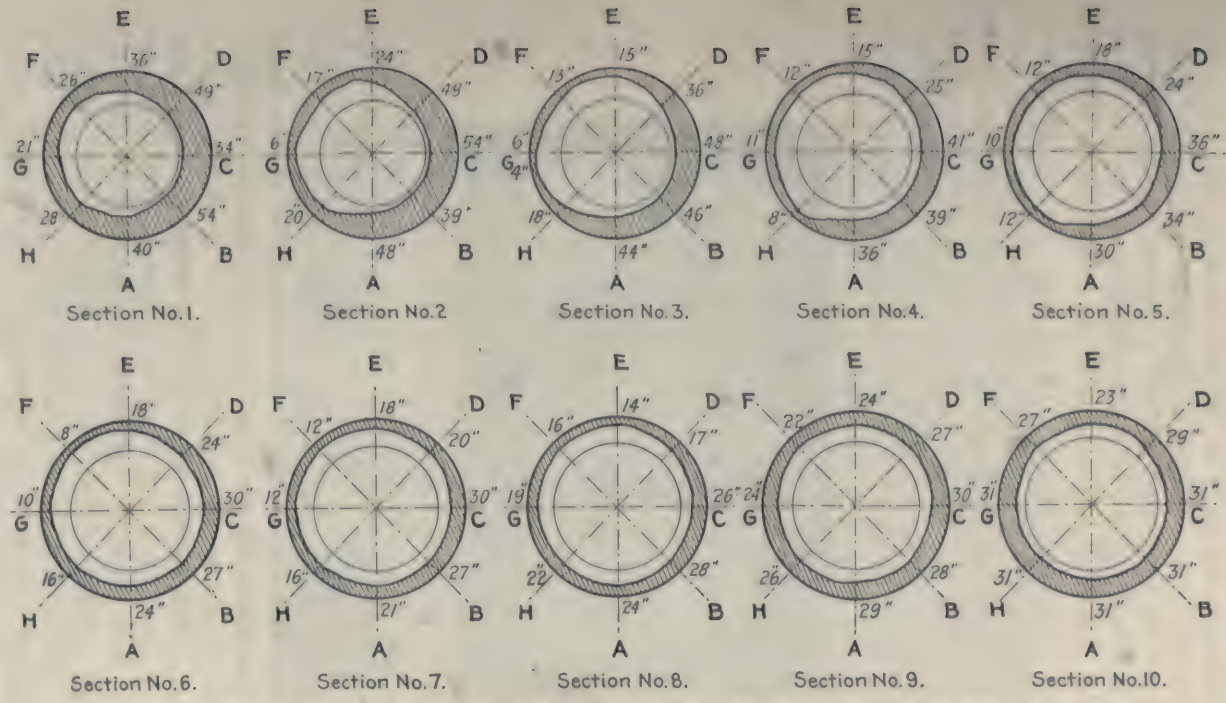

FIG. 5.—Horizontal Lines of Erosion on a Blast Furnace after Six Years' Service—Uniform Wearing.

NOTE.—The letters at the ends of a diameter refer to the corresponding section indicated in the plan view of Fig. 4. The figures at the ends of a diameter indicate the thickness of the brick lining remaining at the ends of that diameter. *For example:* Horizontal section No. 1 shows thicknesses at the ends of the diameter EA equal to 36 and 40 in.; at the ends of diameter FB, 26 and 54 in., etc. These figures will also be found along the horizontal line of section No. 1, in Fig. 4.

cold and say that the result of the cold test was a measure of the performance when hot, did not seem justifiable. Another point was to make the tests such that the work could be done on the brick as received, and not on bricks prepared for the test.

In the study of service conditions, attention was first directed to the top of the blast furnace. The average temperature at the top is about 260° C. The charging of a furnace is a nearly continuous operation, the material charged falling on the large bell, which when lowered allows the material, often wet or frozen, to slide off the bell and strike the walls of the furnace. Immediately the charge starts slipping and grinding its way slowly downward

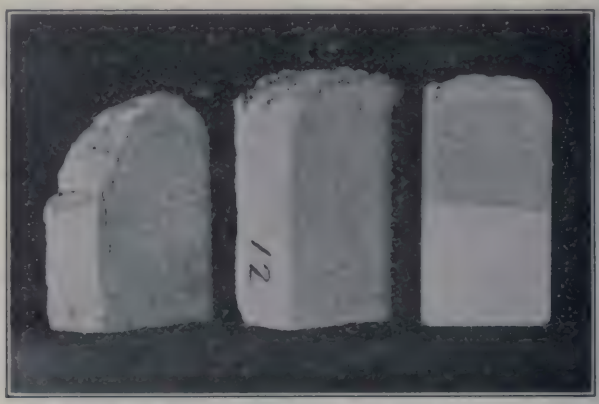

FIG. 6.—Characteristic Spalling-Test Specimens.

past the brick in the wall. These conditions required tests that would imitate impact, abrasion and spalling.

Impact Test.—An impact test was first considered. The rattler and other similar tests were studied, but it was finally decided that some kind of a drop test would be the most practical, and would most nearly approach service conditions. After trying many shapes and sizes of falling weights it was finally decided to use a steel ball $2\frac{1}{2}$ in. in diameter. The brick was heated to 260° C. and the ball allowed to drop on the end of the brick from a height of 2 in., then 4 in., and so on, until the brick broke. The height to which it was necessary to raise the ball to cause fracture of the brick represents a particular quality, namely, resistance to impact.

The comparative strengths of brick to resist impact at ordinary temperatures and at slightly elevated temperatures is interesting. A brick heated to 260° C. and tested was 20 per cent weaker than a brick of the same brand tested at 20° C., and 40 per cent weaker when tested at 540° C. (Fig. 1).

FIG. 7.—Spalling in Roof of Air Flue of Open-Hearth Furnace.

(Note similarity between actual and test spalling.)

Abrasion Test.—In developing an abrasion test it was found that others had abraded brick in the cold by means of an emery wheel. This idea was modified so as to test the brick while hot. A temperature of 260° C. was adopted for top-wall and pipe brick. and 1350° C. for all others. The reason for choosing 1350° C. for the high temperature was that (1) it is a comparatively easy

temperature to reach with a test furnace, and (2) while it might not represent the highest temperature in the furnace, it is above the melting point of cast iron and is applicable to all high-temperature furnaces used about iron and steel plants.

The brick to be tested was heated to 1350° C. and then pressed against a carborundum wheel, pressure and time being constant. When the brick became cold the depth of cut was measured and the result reported as linear inches abraded in 5 minutes. This test developed the interesting fact that bricks are not always the same at both ends, for it was found that sometimes there may be 10 to 20 times as much abrasion at one end as the other (Figs. 2, 3, 4 and 5).

FIG. 8.—Characteristic Slag-Test Specimens.

Spalling Test.—Spalling was next studied. In brick work such a condition is usually the result of thermal changes, often accelerated by mechanical pinching. In actual service only one surface, generally the end or side of the brick, is exposed to the direct action of heat.

After trying various tests it was decided to place the brick to be tested in the wall of the furnace so that only one end was exposed. The testing temperature was 1350° C., and after a series of experiments as to the most satisfactory time for heating, one hour was decided upon, with 25 minutes cooling in a blast of air. To produce a measurable spall at least 30 operations of heating and cooling were required. This was too long, and it was decided to increase the severity of the test by cooling in water

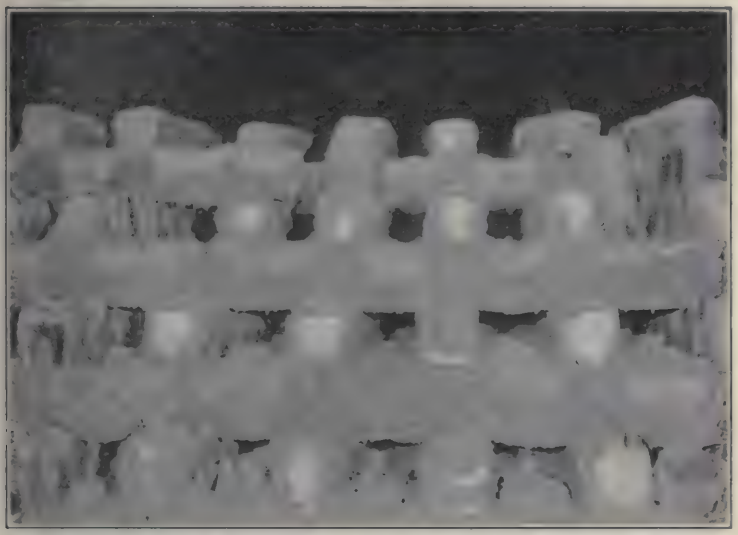

FIG. 9.—Checkers in Gas Chamber of Open-Hearth Furnace, Showing Slagging.

FIG. 10.—Individual Checkers from Gas Chamber of Open-Hearth Furnace, Showing Slag Penetration.

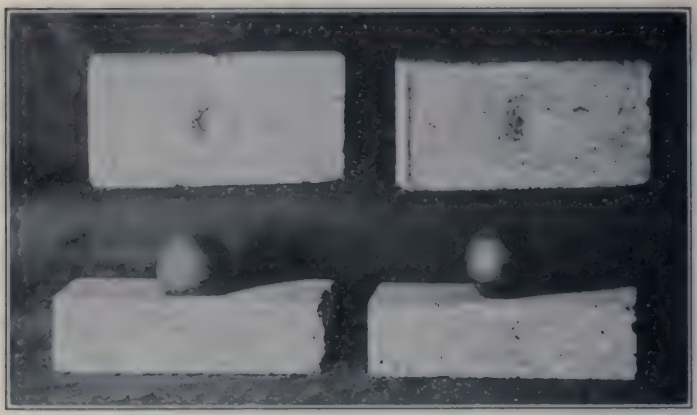

FIG. 11.—Characteristic Compression Test Specimens, Showing Depth of Ball and Corresponding Impressions.

FIG. 12.—Spalling, Accelerated by Compression, of Entire Corner of Air Flue in Open-Hearth Furnace.

for 3 minutes, and so hasten the spalling. A series of tests determined that cooling in water was 3 times as effective as air cooling. The number of operations was accordingly cut down to ten. The amount of spalling is reported as the percentage loss in weight. This test applies only to fire brick (Figs. 6 and 7).

Slagging Test.—The failure of brick work due to the corrosive action of slags is more prevalent than any other cause. Not only is the slag action marked in blast furnaces, but also in open hearths, cupolas, and heating furnaces, and to a lesser degree in mixers, ladles, fireboxes, etc.

During the development of a slag test the slag was first placed on the surface of the brick. It was next confined by the use of

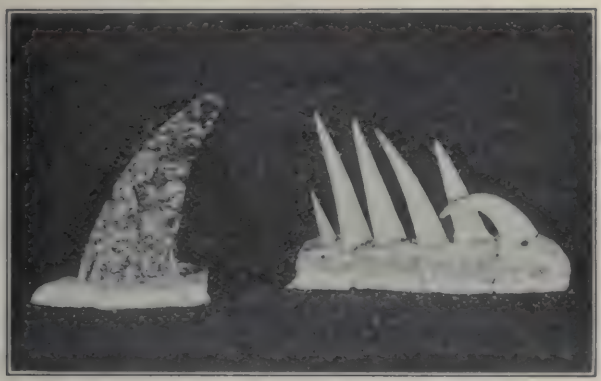

FIG. 13.—Characteristic Softening-Test Specimens.

kaolin rings, but both tests were unsatisfactory. To confine a comparatively large portion of slag to any one portion of the brick it became necessary to drill a cavity in the brick and place the slag therein. Experiments were made to determine the effect of fineness of the slag, time of exposure to heat, and the effect of destroying the surface texture by drilling. To measure the slag penetration the brick is cut through the pockets of the slag and the area of brick penetrated by the slag measured with a planimeter (Figs. 8, 9 and 10).

Compression Test.—The ability of brick work to stand up under loads when highly heated is of great importance. A comparative test consisting of a modified Brinell ball test was

used by heating the brick to 1350° C., removing from the furnace and then forcing a 2½-in. steel ball into the brick under a pressure of 1600 lb. The depth of the impression was then measured when cold (Figs. 11 and 12).

Expansion, Contraction and Fusibility.—These are important qualitites of fire brick. For these tests the usual methods were

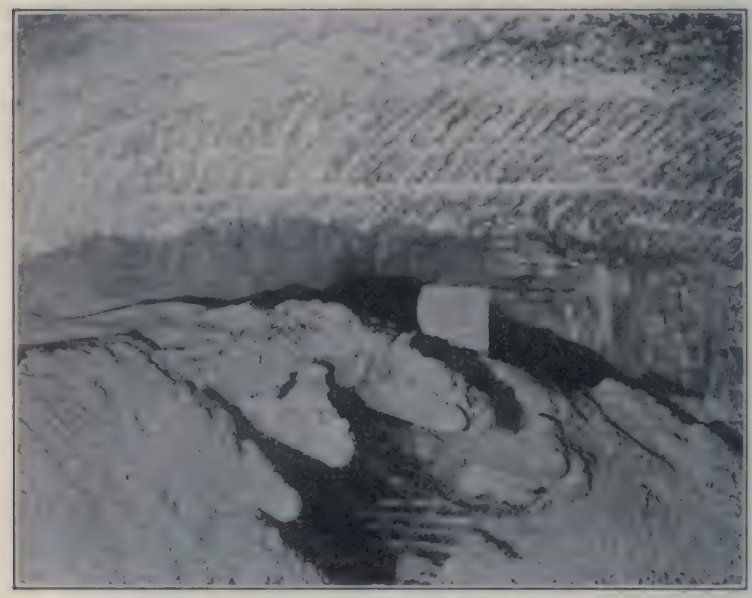

FIG. 14.—Gas and Air Ports inside Open-Hearth Furnace.

(Note slagged condition of walls and roof, and total disappearance of brick around water cooler in foreground.)

followed, and they need not be mentioned further at present (Figs. 13 and 14).

RESULTS OF TESTS.

As a result of a large number of tests following the methods which have been outlined, it was found that fire bricks tended to group themselves according to density. In general, bricks of high density, all other factors being equal, have the most desirable

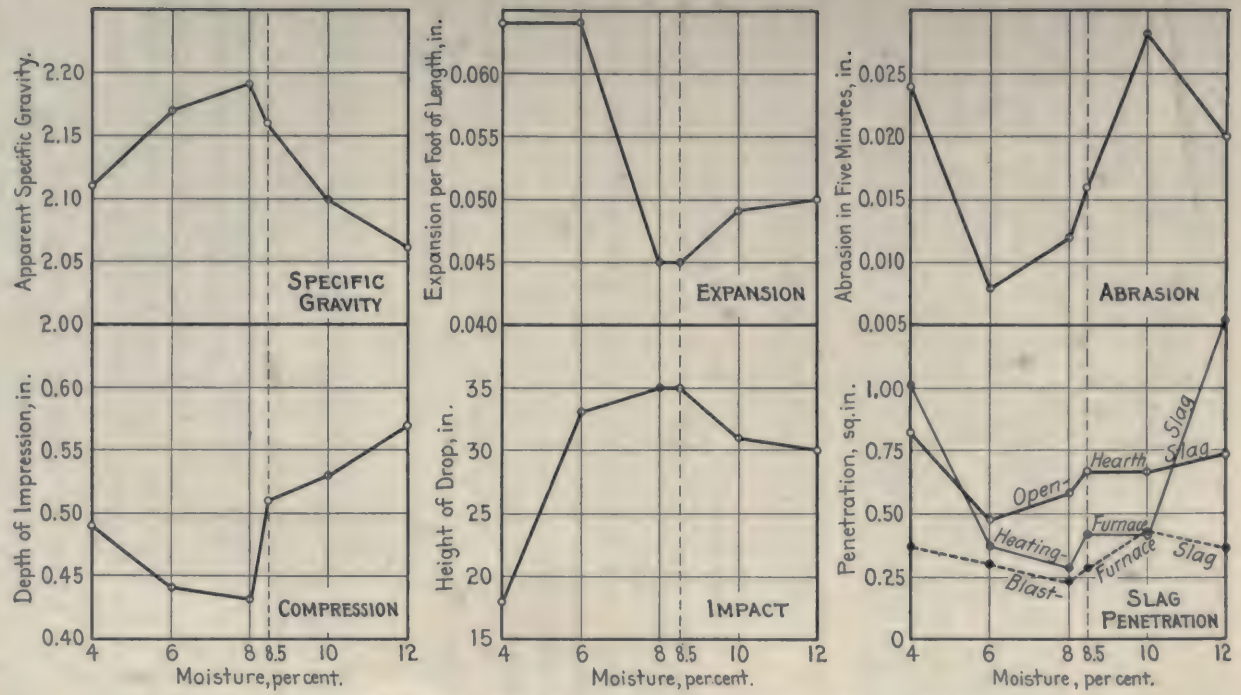

FIG. 15.—Influence of Water Used in Manufacture on Physical Properties of Fire Brick.

qualities, namely, least abrasion, good resistance to slag and least compression. Spalling is very slightly increased but not to a dangerous extent if bricks are not over-burned.

As a measure of density, the apparent specific gravity is used—obtained by weighing and measuring the brick and not by suspension, as is usually done. Results show that various makes of brick have characteristic values for apparent specific gravity and that the method of manufacture greatly influences the density. A series of preliminary experiments had shown the importance of pressure and moisture in producing a dense brick, and this test was followed up by a more elaborate series. A standard fire-clay mixture was used, in which the moisture was

TABLE I.—RESULTS OF TESTS ON FIRE BRICK OF VARYING MOISTURE CONTENT.

Properties Considered.		Percentage of Moisture.					
		4	6	7	8.5	10	12
Apparent Specific Gravity		2.11	2.17	2.19	2.16	2.10	2.06
Compression at 1350° C., depth of impression, in.		0.49	0.44	0.43	0.51	0.53	0.57
Expansion per foot of length at 1350° C., in.		0.064	0.064	0.045	0.045	0.049	0.050
Impact at 260° C., height of drop, in.		18	33	35	35	31	30
Abrasion at 1350° C., in five minutes, in.		0.024	0.008	0.012	0.016	0.028	0.020
Slag Penetration, sq. in.	Blast-furnace slag	0.37	0.30	0.24	0.28	0.43	0.36
	Open-hearth slag	0.82	0.48	0.58	0.66	0.66	0.74
	Heating-furnace slag	1.05	0.35	0.29	0.42	0.42	1.26

varied from 4 to 12 per cent, and all bricks were pressed at 2000 lb. per sq. in., then burned in a regular kiln. Results of tests applied on this series are shown in Table I and Fig. 15.

Power-pressing a brick during manufacture up to 1500 lb. per sq. in. has been found to materially improve the quality of most fire brick; beyond that figure very little improvement is made. A comparison of tests on a high-pressure, low-moisture brick and a hand-made, repressed, ordinary-moisture brick, both brick of the same mixture, is given in Table II.

The laboratory methods suggested have been developed mainly with reference to bricks to be used in iron and steel plants. When modified they are applicable to almost any condition of

service to which the bricks will be subjected. If a brick is to be used in a boiler setting, copper or lead smelter, calcining or roasting furnaces, the test should imitate these conditions, in order that the results be comparative. Conditions of service must be studied to learn what is demanded of a brick.

When a laboratory test has been developed it must be confirmed by actual service tests. To compare the life of two similar furnaces built of different materials, or even the same furnace built of different materials at different times, does not necessarily mean that the material showing the longest life is the better, since the operating conditions may have been decidedly different.

TABLE II.—RESULTS OF TESTS ON A HIGH-PRESSURE, LOW-MOISTURE BRICK AND A HAND-MADE, REPRESSED, ORDINARY-MOISTURE BRICK.

Properties Considered.		Kind of Brick.	
		Pressure of 1500 lb. per sq. in., Moisture, 7 per cent.	Hand Made, Repressed, Ordinary Moisture.
Compression at 1350° C., depth of impression, in		0.29	0.55
Expansion per foot of length at 1350° C., in		0.062	0.050
Impact at 260° C., height of drop, in		45	28
Abrasion at 1350° C. in five minutes, in		0.02	0.04
Spalling, loss, per cent		9.4	10.6
Slag Penetration, sq. in.	Blast-furnace slag	0.27	0.56
	Heating-furnace slag	0.26	0.67

Conclusions can only be drawn when all conditions except brick quality are the same.

To derive the greatest benefit the consumer and manufacturer must work together. The manufacturer's interest must not cease with the shipment of his product. The consumer should provide all reasonable facilities for the manufacturer to study his product in actual service. It is only by such cooperation or exchange of information that the best results can be obtained.

In this paper only the main points of the tests have been dealt with, while a more detailed description of apparatus and methods is appended.

APPENDIX.

METHODS OF TESTING REFRACTORY FIRE BRICK.[1]

Impact Test.

Construction of Apparatus.—The machine for determining the resistance to impact consists of two vertical uprights 15 ft. high, supporting two vertical and parallel 1½-in. angle irons. The angle irons are placed so that their enclosed angles face each other and are braced 3⅜ in. apart measured from the inside of the angles, so as to allow a ball 2½ in. in diameter to fall freely between them, the sidewise movement of the ball not to exceed ¼ in. These angle irons are adjusted so that bricks of different heights may be placed under them and are marked, beginning at the lower ends, so as to clearly designate successive heights of 2 in. The test specimen is placed directly below the angle irons on a steel block of sufficient size to give a solid foundation. The upper face of the block is perfectly smooth and at right angles to the angle irons. The block is 6½ by 15 by 12 in. and weighs 332 lb. The falling weight is a steel ball 2½ in. in diameter weighing 2.34 lb.

Operation of Test.—The brick to be tested is first carefully inspected, and, if necessary, ground on a carborundum wheel so that it sets firmly on one of its ends. On the other end of the brick the diagonals are then drawn to locate the center. The brick is now placed in a cold furnace and heated at a uniform rate from atmospheric temperature to 260° C., so that at least 1 hour is required to reach the final temperature. The final temperature (260° C.) is maintained for not less than 3 hours.

The brick is now ready for the test and is placed on the steel block, so that the ball between the angle irons touches the brick at its marked center. Iron blocks are placed around the brick until they are nearly level with the top of it, to prevent sidewise

[1] At the present time the machines and apparatus here described are in a state of development and while they have been standardized for the test, the machines and methods themselves are not necessarily final.

movement, but the brick is not wedged. The ball is now dropped from a pair of tongs on the end of the brick at successively increasing heights of 2 in. until the brick fractures. Any fracture radiating 1 in. or more from the point of contact of the ball is considered a fracture, and the distance in inches of the last drop is the result of the test. If the ball is allowed to drop again from the last height the brick gives off a dead or broken sound. The clear ring of a sound brick has disappeared.

COMPRESSION TEST.

Construction of Apparatus.—The machine for determining the resistance to compression is a modified Brinell ball testing machine and consists of a 4-in. I-beam, 10 ft. long and weighing 75 lb., so arranged as to act as a second-class lever arm; that is, one end is the fulcrum and at the other end the weight is applied so as to produce the compression on the brick placed 6 in. from the fulcrum. All bearings are knife-edged. The fulcrum end of the bar is mounted between two vertical uprights, 10-in. channel irons, and so arranged that the height of the fulcrum may be adjusted by a hand wheel to suit bricks of varying thickness. Six inches from the fulcrum towards the weighted end of the bar a spherical depression marks the location of the ball. The ball is a steel ball $2\frac{1}{2}$ in. in diameter weighing 2.34 lb. The brick to be tested rests on a solid foundation directly below the depression. The bar alone produces a pressure of 850 lb. on the ball, and the balance of the weight is added at a uniform rate of 25 lb. per second by means of dry sand which flows from a hopper into a bucket attached to the end of the bar. Bar and sand together produce a total load on the ball of 1600 lb.

Operation of Test.—The brick to be tested is placed in a cold furnace and heated slowly to 1350° C. The heating as a convenience is done over night, at a uniform rate not to exceed 260° C. per hour. Total time of heating should in no case be less than 6 hours. The brick before testing should be maintained at 1350° C. for not less than 3 hours.

The brick is now removed from the furnace and placed flat on the foundation beneath the bar, and the bar lowered so that when the ball is in position in the depression, it is also in contact with the center of the upper surface of the brick. The weighted

end of the bar is then gently released and the flow of sand immediately started. The time required to remove the brick from the furnace and apply the load is less than 1 minute. The pressure is maintained for 5 minutes, starting from the time the bar is released. The brick is then removed from the machine and allowed to cool. When cold the depth of penetration produced by the ball is measured by placing centrally over the depression a flat rectangular plate 3½ in. square (¼ in. thick), at the center of which is mounted at right angles to it a Brown & Sharpe micrometer depth gage. The lower end of the stem of the micrometer is rounded so that it has the same radius as the 2½-in. ball. The depth at the center of the depression is measured to 0.001 in.

ABRASION TEST.

Construction of Apparatus.—The abrasion testing machine consists of a carborundum wheel mounted on a suitable stand and revolving vertically. In front of the wheel and resting on the stand is an L-shaped plate 6½ by 16 by ¼ in. resting on roller bearings, and forced towards the wheel by means of a right-angle lever arm, and suspended weights. The upper surface of this plate is 1¼ in. below the horizontal radial line drawn from the center of wheel. All points of contact on the lever arm are knife-edged and friction is reduced to a minimum. The carborundum wheel is 18 in. in diameter, has a 2-in. face and 1½-in. center hole, and is purchased from the Carborundum Co., grit 16; grade J; bond G-5. The wheel is motor driven and has a surface speed of 1640 ft. per minute and is to be redressed when the corners wear away ¼ in. of their original radius, or as soon as the face ceases to be at right angles to the side of the wheel. The pressure of the brick against the wheel is 10 lb. per sq. in. of brick in contact with the wheel, excluding the pressure required to move the brick.

Operation of Test.—The brick before testing is ground cold to a uniform thickness of 2.35 in. for 2½-in. brick and for 2.80 in. for a 3-in. brick, and the ends ground so that the testing wheel cuts through this thickness. The depth of this preliminary cut is measured at its center, using the rectangular plate described under the compression test. The brick prepared for test is heated the same as under the compression test, except in the case of top-wall and pipe brick, which are heated the same as for the impact test.

The brick is then removed from the furnace, placed flat on the plate in front of the wheel so that the wheel enters the preliminary cut, and is then abraded for 5 minutes. The time is taken with a stop-watch. In the case of soft brick which abrade very rapidly the test is run for a fraction of the period, and then calculated to the 5-minute period. The brick is now returned to the furnace and reheated for not less than 1 hour. The test is then repeated on the other end of the brick. When cold the depth of cut on both ends is measured, and the difference between the preliminary and the final cuts is the result reported.

SPALLING TEST.

Operation of Test.—Preparatory to the test the furnace is heated to 1350° C. and held at this temperature for about one hour. The bricks to be tested are dried at 100° C. for at least 5 hours, weighed, and then placed in the doorway of the furnace, their ends flush with the inside of the wall. In case a sufficient number of bricks are not being tested to entirely fill the doorway, the remainder of the space is filled in with other bricks to insure heating the ends only. The bricks are now heated for 1 hour and plunged separately into 2 gallons of water at 20° C. to a depth of 4 in. and held there for 3 minutes. The bricks are then removed and allowed to dry for 3 minutes in air. To indicate 3-minute periods a stop-watch is used. The bricks are then placed in the door-way of the furnace as before and the operation repeated for a total number of ten times. The bricks are now dried at 100° C. for at least 5 hours and all particles which can be easily broken off with the fingers removed. The bricks are then weighed and the percentage of loss calculated from the original weight.

SLAGGING TEST.

Operation of Test.—In preparing the sample for the slagging test, the 9 by 4½-in. unbranded side of the brick is bisected by a line across the narrow dimension and diagonals drawn on each half. At each intersection of the diagonals a circular cavity is drilled 2½ in. in diameter and ½ in. deep at the sides. The drills are $\frac{5}{16}$ in. thick and 2½ in. wide, measured diagonally across opposite corners, while the point of the drill includes an angle of

150 deg. A template is made of $\frac{3}{16}$-in. sheet steel to insure that all drills are ground properly. To insure a standard depth of cavity, two inverted U-shaped pieces of metal are clamped to each drill when in use and adjusted so that drill cannot enter the hole more than $\frac{1}{2}$ in., measured on the side of the drill. The cross-section of the cavity passing through the deepest point has an area of 1.7 sq. in., and is checked up for size and depth by a template. The pressure used in drilling must not be so severe as to partially crack the brick.

After the cavities have been drilled and cleaned of loose material the bricks are placed level in a cold furnace and heated as in the compression test. When 1350° C. is reached 35 g. of standard blast-furnace slag are placed, by means of a long rod with a spoon-shaped end, in one cavity, and 35 g. of standard heating-furnace slag in the other cavity. The slags used shall have approximately the following analysis and melting points:

	BLAST-FURNACE SLAG.	HEATING-FURNACE SLAG.
Silica (SiO_2), per cent	38.0	35.0
Iron (Fe), per cent	1.5	44.0
Manganese (Mn), per cent	1.0	0.5
Alumina (Al_2O_3), per cent	14.5	6.0
Lime (CaO), per cent	42.0	1.5
Magnesia (MgO), per cent	2.0	0.5
Sulfur (S), per cent	1.0	....
Melting Point, deg. Cent	1260	1260

The fineness of the slag is such as to pass a 40-mesh sieve. The bricks are kept at a temperature of 1350° C. for two hours after the slag is added, at the end of which time the furnace is allowed to cool, or the bricks may be removed from the furnace and allowed to cool in air. The bricks when cold are sawed lengthwise at a right angle to the unbranded side, so that one side of the cutting wheel cuts through the center of the drilled cavity, and this area of slag penetration, together with the cavity is measured with a planimeter. The area of the original cavity is subtracted, giving the result in square inches of slag penetration. The wheel used for sawing the brick is a carborundum wheel 12 in. in diameter; $\frac{1}{8}$ in. thick; $1\frac{1}{2}$-in. center hole; grit 16; bond NCH.

SOFTENING TEST.

Operation of Test.—The test specimens for determining the softening temperature are cut by means of the saw used in slagging test from the corner of the brick, in the shape of a triangular pyramid similar to that of a Seger cone. They are 3 in. high, and the base should not be less than 1 in. on the side. The test specimen, together with a series of Seger cones, is set in some good refractory material which will not flux with the sample.

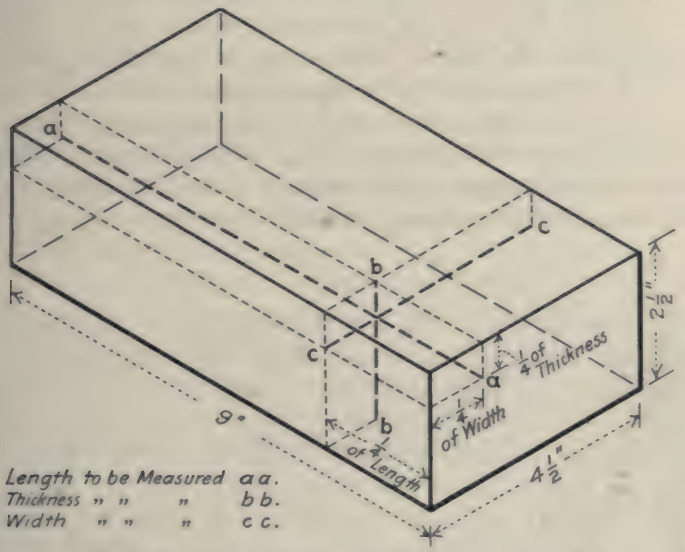

FIG. 16.—Axis along which Measurements are taken in Determining the Size of a Brick.

The series of cones used includes numbers both below and above the cone at which the sample is supposed to soften. The whole is placed in a gas-fired furnace and heated at a uniform rate, not to exceed 600° C. per hour. The softening temperature is considered that at which the specimen either bends double, sags, or puffs out of shape and is determined by Seger cones.

EXPANSION AND CONTRACTION.

Construction of Apparatus.—The machine used for the measurement of expansion and contraction consists of a microme-

ter mounted on a steel post, which in turn is mounted on a steel plate 24 by 6 by ½ in. To the stem of the micrometer is fastened a steel disk 1 in. in diameter and ⅛ in. thick. In line with the micrometer is a second steel post on which is mounted a ¼-in. sliding steel rod with a similar circular disk, as with the micrometer. The rod can be adjusted so as to determine measurements up to 14 in. To insure that this rod is properly set standard bars 3, 5, 9, 11 and 13 in. long are used.

Operation of Test.—In preparing the brick for the test, the ends of the brick are ground so as to be parallel to each other at right angles to the sides. The length is then measured cold. The brick is then heated as in the compression test,—or as for the impact test in the case of top-wall and pipe brick,—removed from the furnace and measured hot. The removing from the furnace and measuring usually requires about 30 seconds. The result is calculated to inches per linear foot.

SIZE.

Operation of Test.—The size is determined by the use of the micrometer described under expansion and contraction. All measurements are made while the brick is at atmospheric temperature. Small particles of sand, clay, or foreign matter adhering to the brick are removed before measuring. The locations for taking the measurements are shown in Fig. 16. If the bricks are of a different size than a standard 9-in. brick the measurements are made in the same way but at a point of one-fourth of their respective dimensions.

DISCUSSION.

MR. J. S. UNGER (*presented in written form and read by the author*).—The paper just read brings forcibly to the minds of those interested in refractory materials, the importance of having some tests with which to determine what particular physical properties a brick must possess, and to show its fitness or unfitness for use in a particular type of furnace or operation. Mr. Unger.

The suggestion of the authors that a test should imitate service conditions, but be of such severity as to show results in a short time, is a radical departure from the methods commonly employed in testing bricks. The user of refractory bricks knows that a certain part of his furnace fails more rapidly than another, and that two apparently identical furnaces operating under the same conditions may show a different life. Such results can not always be attributed to the quality of bricks used, as the bricks may be of the same kind. The bulk-head walls of an open-hearth furnace fail first, followed by the roof, then the side walls above the hearth. A blast-furnace lining shows the greatest wear just above the mantel. Regenerator chambers and hot-blast stoves fail by a clogging up of the air or gas passages.

Bricks rarely fail from one cause alone, such as fusion, slagging, or abrasion. A combination of several causes is usually present, which results in a more rapid deterioration of the brick than would any one alone.

A remarkable feature of the abrasion test is that the same brick may show many times as much abrasion at one end as the other, which would indicate some great irregularity in its manufacture.

Under the spalling test water cooling has been substituted for air cooling. It would seem reasonable to suppose that the moisture and steam generated, both of which tend to disintegrate the brick very rapidly, could hardly be compared with air cooling even if repeated three times. I feel that the method proposed should be given most careful comparative study on different kinds of bricks to determine if it be truly representative. In some furnace operations the charge is rapidly cooled

Mr. Unger. off by water, which cools the brick work as well. Such rapid cooling however is not the regular practice, as most furnaces are allowed to cool in the air. If there be a true relation between air and water cooling, the latter method of cooling is preferable as it must shorten the time of making a test.

It is to be regretted that the authors have not had the space to give a little more detailed description of the compression test in Fig. 11, and the fusion or softening test in Fig. 13. I am interested in knowing how the cooling of the skin of the brick in the compression test affects the test, or if it may be disregarded.

To my mind the most important points in the paper are the curves in Fig. 15 and Table I, showing the effect of the water used in the manufacture of bricks, and Table II showing the effect of pressure in making a brick. The correlation of the apparent specific gravity with the other physical properties is one of great importance to the brick manufacturer, as it furnishes him with a means of quickly, although perhaps approximately, determining some of the qualities of his brick.

Any information which enables the brick manufacturer to improve his product by proper moisture content, pressure or other agencies is most valuable, as his choice of the best raw materials to make a suitable mixture is usually limited by locality or cost.

I hope the authors may find it convenient to present additional data, or possibly proposed specifications, in some future paper, as I consider this paper a very valuable contribution to this Society and to both the maker and user of refractory bricks.

Mr. Stone. MR. GEORGE C. STONE.—I am glad to be able to confirm the conclusion of the authors that the denser brick, as a rule, is the better. This completely agrees with our results, and we have done a good deal of testing on similar lines. In regard to the method of determining corrosion by slags, we have found it impossible to get concordant or good results except by working on the natural surface of the brick; if we cut the brick, the results obtained are not as good. Our usual method is to take a piece of brick having a natural surface about 2 in. square, mold a small block of the corrosive material (about a ½-in. cube), mix it with water containing a little dextrine and melt it on the brick.

I notice the authors state in the appendix to their paper that the blast-furnace slag and heating-furnace slag melted at the same temperature. I tried slags of the composition mentioned in the paper and found that the heating-furnace slag melted at about 1100° and the blast-furnace slag at 1260° C. Mr. Stone.

MR. W. H. FULWEILER.—I wish to congratulate the authors upon their courage in breaking away from the conservative methods of testing refractory materials and in devising methods which aim straight at the point. Mr. Fulweiler

In connection with the hardness test, it may be open to some argument as to just how the results of this test would indicate the ability of the brick to withstand a uniformly distributed load at high temperatures. It would seem that with a material as heterogeneous as fire brick, the effect produced by pressing a relatively small ball into the surface would not give results that were directly comparable with those obtained where the load was uniformly applied. I am referring now more directly to that type of test heretofore in use known as the load test, where a definite load is applied and the brick heated to specified temperatures. In other words, the authors have not shown, if that has been their intention, just how the results of their hardness test are correlated with the load-carrying ability of refractories at high temperatures.

In connection with the test for slagging action, it is difficult to understand why the outer skin of the brick was removed. Laboratory tests have indicated that the removal of the outer skin of the brick quite destroys any regularity in the results where bricks are exposed to the slagging action, and we know from practical experience that in cases where the outer skin has been removed, bricks have failed more rapidly than when the original skin is left on.

With reference to the abrasion test, some experiments seem to indicate that with any given pressure on the brick, samples may be arranged in a certain order, but if this pressure is changed, either decreased or increased, these same bricks would no longer arrange themselves in the same order. For each particular brick there seems to be a critical pressure that will cause the maximum abrasive action, and the pressure employed is, therefore, a very important factor.

Mr. Bleininger. MR. A. V. BLEININGER.—Discussing these new tests, I should like to inquire as to the magnitude of the temperature change involved in taking these bricks from the furnace and placing them in the pressure device. It seems to me to be important that something should be known concerning the temperature drop in this manipulation. It might be easily possible that sometimes more time is consumed in getting the brick in position, with a correspondingly greater drop in temperature. It might be feasible to construct an apparatus in which the pressure is applied to the brick while it is still in the furnace.

I wish to compliment the authors on the bold conceptions which they have utilized.

Mr. Purdy. MR. R. C. PURDY.—I have been very much interested in this paper, because the Norton Co., with which I am connected, is a large user of brick and we also manufacture a large quantity of fire-clay goods. We find that no laboratory test that has yet been devised will tell what a brick will do in service. I think that emphasis should be placed upon the service test. We have received brick, claimed to have given excellent results under severe conditions, which, under our apparently less severe conditions, have failed to give satisfactory service.

Our principal test is to build the bricks into the equivalent of a bridge wall over which the flame of soft coal passes, tie the bridge to prevent expansion, so that it brings upon itself considerable pressure, and then apply weights upon the top of the wall. Bricks that fail there in the course of two months cannot be used by us in the flue walls of the kiln. I think this is a very severe test, but we find that it is absolutely necessary in order to obtain brick that will stand under the heavy load in our kilns. I do not believe that a laboratory test can be devised which will give absolute surety of a service result. I commend the authors on recognizing the fact that a service test is very important.

Mr. Nesbitt. MR. C. E. NESBITT.—With regard to Mr. Unger's remark in respect to the cooling of the skin of the brick during the compression test, I might say that it is very slight; but the rate of cooling is the same for all tests, hence, comparative results are obtained.

Mr. Nesbitt.

The softening test specimen shown in Fig. 13 of the paper is cut from a corner of the brick with a thin carborundum wheel, and is of sufficient size to be truly representative of the brick. It is heated in a furnace to the softening point together with Seger cones, the usual precautions regarding the rate of heating etc., being rigidly adhered to.

In regard to specifications, it is quite possible that proposed specifications may be drawn up in the near future which can be met and will better the quality of the brick considerably.

With reference to Mr. Stone's remarks about the slagging test and its effect on the natural surface of the brick, we have made tests along this line and find that the effect of the skin of the brick is to lower the results 15 per cent. In regard to the melting point of the heating-furnace slag, I might say that there are all kinds of slag, and their melting point varies somewhat. The slag we are using at the present time has the composition and melting point indicated in the paper.

Referring to Mr. Fulweiler's remark about the load test, I would say that we have already checked Mr. Bleininger's test against our compression test, and were very much surprised not only to check him, but to do so in the hundredth place on four or five different brands of brick. We expect to do more work along this line. As to the abrasion test, the pressure used was as heavy as the brick would take and still permit the operation of the wheel without glazing the brick. We wanted the test to be as severe as possible, so that we could get results in a short time which in actual service would require a long time to obtain.

Answering Mr. Bleininger's inquiry about the cooling of the brick during the compression test, I might say that it seldom takes more than 30 seconds to place the brick in position after removing it from the furnace. In testing, the movement of the ball is practically completed in two minutes, although the test is continued for a five-minute period.

Mr. Purdy is quite right in his remarks that laboratory tests should be followed up by service tests, but I do not quite agree with him that some laboratory tests cannot be devised that are indicative. At the present time we have several special lots of brick from which we have obtained results by laboratory methods, and we are now proving these results in actual service.

THE SPECIFIC GRAVITY OF NON-HOMOGENEOUS AGGREGATES.

By Prévost Hubbard and F. H. Jackson, Jr.

SUMMARY.

The object of this investigation was to study methods in common use, or those which gave promise of being most satisfactory, for the determination of apparent and true specific gravity of mineral aggregates, with a view to ascertaining if possible what method is most generally applicable to all classes of materials, and also the most accurate. The methods studied were as follows:

1. The ordinary displacement method as conducted by the U. S. Office of Public Roads and Rural Engineering;
2. The Chapman method for single specimens;
3. The Goldbeck method;
4. The Hubbard-Jackson method;
5. The Chapman wire-basket method;
6. The Le Chatelier method for fine aggregates; and
7. The Jackson method for fine aggregates.

Descriptions of these tests in detail, together with illustrations, are given. Sixteen samples of crushed rock, three samples of crushed slag, a sample of gravel and three samples of sand were used in this investigation.

As the result of the work accomplished the following conclusions were drawn:

1. In the case of rock and slag an appreciable variation may exist between apparent specific gravity and true specific gravity, depending upon the absorption of the material.

2. It is impracticable by any of the methods studied to determine the apparent specific gravity of samples composed of fragments smaller than ¼ in. in diameter.

3. Methods employing single test specimens are not safe to use for determining the specific gravity of non-homogeneous aggregates, even when the average results of three apparently representative samples are taken.

4. In the case of non-homogeneous aggregates consisting of fragments of not less than ½ in. in diameter, all of the methods employing 1000-g. samples are satisfactory and can ordinarily be depended upon to give check results by different operators working upon the same sample to within 1 in the third significant figure.

5. When determining the specific gravity of extremely non-homogeneous aggregates, it is recommended that the average of not less than three tests, made upon different 1000-g. samples, be reported.

6. When it is desired to obtain as nearly as possible the apparent specific gravity of aggregates consisting of a mixture of coarse and fine particles, it has appeared advisable to separate a weighed sample of the material by means of a ¼-in. screen, and to make an apparent-specific-gravity determination upon not less than 1000 g. of the coarse fraction and a true-specific-gravity determination upon not less than 50 g. of the finer fraction. The approximate apparent specific gravity of the whole sample may then be calculated from the results obtained.

THE SPECIFIC GRAVITY OF NON-HOMOGENEOUS AGGREGATES.

By Prévost Hubbard and F. H. Jackson, Jr.

The determination of the specific gravity of material composed of mineral particles is not ordinarily considered a difficult matter, and until recently but little attention has been paid to the subject in so far as non-bituminous road and paving materials are concerned. Certain discrepancies in results obtained in the laboratories of the U. S. Office of Public Roads and Rural Engineering have, however, indicated to the authors that the entire subject of the specific gravity of these products should be investigated with a view to ascertaining, if possible, what method is most generally applicable to all classes of materials, and also the most accurate.

The method most commonly used in determining the specific gravity of such substances as rock is to first weigh a specimen of rock in air and then in water, and from these results to calculate the specific gravity by the usual formula applied to what has been termed the displacement method. It is of course evident that unless the average of a large number of determinations upon different specimens is taken as representing the specific gravity of the material, any method employing a single specimen must, of necessity, be accurate only for material which is of a homogeneous nature. Certain rocks, as well as many slags, gravels, etc., are composed of non-homogeneous material, so that a single small specimen can only represent by accident the average of the material as a whole.

In addition to lack of homogeneity in the material itself, there must also be considered lack of homogeneity in the size of particles present in a given lot of material, such as crushed rock, crushed slag and gravel. Both kinds of non-homogeneity may exist in a sample of material whose specific gravity is desired. In such case it may be found that in general all particles above a certain size are more or less alike in character, and that particles

below this size vary greatly from these larger particles, as well as among themselves.

Another factor which must be considered is the difference which often exists between the specific gravity of a mass of material and the specific gravity of the substance of which the mass is composed. For instance, it is evident that a rock which is sufficiently porous to absorb water, as almost all rocks do to a greater or less extent, must of necessity when dry contain spaces filled with air. The specific gravity of the mass then, including the air spaces, is less than that of the substance of which the mass as a whole is composed. The term "apparent specific gravity" has been applied in the first instance and "true specific gravity" in the second. It is evident that the apparent specific gravity may be less than or equal to the true specific gravity, but can never exceed it.

It is often desirable to ascertain the apparent specific gravity of a material. When this is so, it would then seem reasonable to suppose that the most accurate determination of apparent specific gravity could be obtained from determinations made upon particles of the material as it is purchased and used. The reason for this is, that as the size of particles is reduced the proportion of voids or air spaces in the structure of the material becomes reduced, for if reduction were carried to the point of destroying the structure, a large number of mineral particles would be obtained free from all voids or air spaces, and the average specific gravity of all these particles would then represent the true specific gravity of the substance composing the original mass.

The converse is true regarding a determination of the true specific gravity of a material. In other words, as the size of particles is decreased it should be easier to determine the true specific gravity than where the original structure is more nearly preserved. It may be argued that in any displacement method, by allowing a specimen to remain in water until absorption is complete, the voids will be filled, and if in some manner the water filling these voids is measured, a correction may be obtained which will make possible the determination of the true specific gravity of the material. It is probable, however, that this is not the case, as the absorption of a large fragment may be

relatively less than that of a small fragment, owing to the fact that water first absorbed from the outside may imprison air which is present within the structure and prevent its escape. From a mechanical standpoint, however, there is a limit in connection with the fineness of particles best suited to obtaining true specific gravity, namely, the fact that a mass of very fine particles is more apt to entangle air, which cannot be removed by agitation, than is a mass of larger particles. By any displacement method, therefore, the entangled air would vitiate the determination to some extent.

In accurately determining the amount of absorption of a mass of mineral matter where the absorption is weighed, there must also be a certain limit to the size of particles used, for it is necessary to surface-dry these particles after the absorption is complete before weighing them, and any error resulting from imperfect drying must be relatively greater for a mass of very fine particles than for the same weight of larger particles, owing to the tremendously greater surface area exposed. Moreover, if surface-drying itself is perfect, where a great number of fine particles are surface-dried the evaporation of the water absorbed by some particles is likely to proceed while the remainder are being dried.

Where extremely accurate determinations are not required, some of the factors discussed above become negligible as applied to certain types of materials, but in other cases they may prove to be quite important. The necessary degree of accuracy of a determination of the specific gravity is a matter of question, but as such determinations are seldom reported to less than the third significant figure, a single unit of which, applied to road-building materials, may represent more than 0.333 per cent, it is presumed that necessity for determinations accurate to this point are usually desired. This is certainly the case in connection with the use of the specific-gravity determination in ascertaining the voids in pavements constructed of rock, gravel or sand, bound together with a bituminous material.

With these facts in mind the authors have studied seven methods of determining the specific gravity upon a wide variety of types of material. These methods were either in common use or gave promise of being most satisfactory. A study of these methods follows, both from the standpoint of accuracy and general practicability under ordinary working conditions.

The tests were all made by operators of average experience in work of this nature. Routine samples of rock, slag, gravel and sand, which had been submitted to the Office of Public Roads and Rural Engineering for determinations of their road-building properties, were used. Five methods (Nos. 1 to 5) were selected for determining the apparent specific gravity of coarse aggregates and three methods (Nos. 4, 6 and 7) were used for determining the true specific gravity of fine aggregates. The exact procedure, together with a description of the apparatus used, is given in each case.

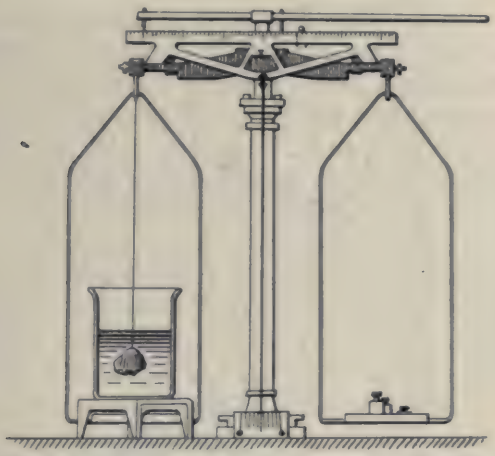

FIG. 1.—Apparatus for Determining Specific Gravity, Methods Nos. 1 and 2.

DESCRIPTION OF METHODS.

Method No. 1.—This method has been used in the laboratory of the Office of Public Roads and Rural Engineering since its organization. A fragment of rock, weighing approximately 10 g. and representing as nearly as possible an average of the sample examined, after being dried to constant weight in a hot-air bath, was cooled, weighed in air and then in water on a chemical balance sensitive to 0.001 g., the specimen being suspended from a hook on one of the beam supports by means of a thin silk thread, as shown in Fig. 1. Inasmuch as the accuracy of the determination of the apparent specific gravity of rock by this method depends

upon the ability of the operator to obtain the weight in water before any appreciable absorption has taken place, the specific gravity of a trial specimen was first obtained and then the approximate weight of the true test specimen in water calculated by proportion. This weight was then placed on the scale pan previous to the immersion of the true test specimen. Calling the weight in air A and the weight in water B, the apparent specific gravity is calculated by this method as follows:

$$\text{Apparent Specific Gravity} = \frac{A}{A-B}$$

Method No. 2.—This method of obtaining apparent specific gravity has been suggested by Mr. Cloyd M. Chapman, and is designed to eliminate errors caused by the absorption of water by the test specimen while weighing. As this method calls for a test specimen of between ½ and 1 in, in size, which is about that used in method No. 1, the same specimens were used in both methods. The sample, after thorough drying, was first weighed in air as in method No 1. It was then immediately immersed in water and allowed to remain for ten minutes, at the expiration of which time it was weighed immersed in water, allowed to remain in water one additional minute, and again weighed, the weighings being repeated each minute until a gain in weight of less than 0.01 g. was noted after one minute. The specimen was then surface-dried with a filter paper and immediately weighed in air. Calling the weight of the dry specimen in air A, the weight in water after constant weight had been attained B, and the weight in air surface-dried C, the apparent specific gravity is calculated as follows:

$$\text{Apparent Specific Gravity} = \frac{A}{C-B}$$

Method No. 3.—This method consists in weighing the water displaced by a sample of rock or slag weighing approximately 1000 g. The apparatus was devised by Mr. A. T. Goldbeck for use with non-homogeneous materials. The vessel used for displacing the water is shown in Fig. 2. It consists of a galvanized-iron cylinder closed at one end, measuring 5 in. in diameter and 10 in. in height. A brass spout ½ in. in internal diameter is soldered into the side of the cylinder at a point 6 in. from the bottom. The spout is inclined at an angle of 2 deg. with the

horizontal and is 3 in. long. In determining the apparent specific gravity, a sample of the material under examination weighing about 1000 g. was broken by hand into pieces which passed a $1\frac{1}{2}$-in. circular opening, but which did not pass a $1\frac{1}{4}$-in. circular opening, dried to constant weight, weighed to the nearest 0.5 g. and immersed in water for 24 hours. It was then surface-dried

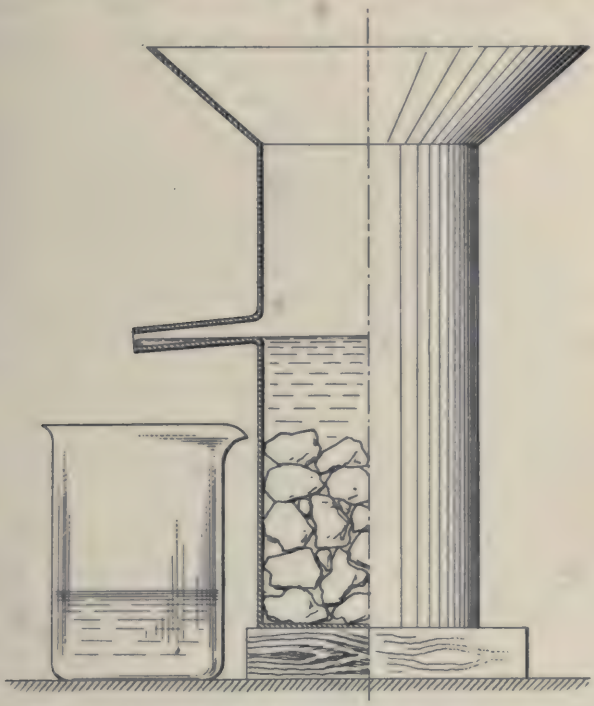

FIG. 2.—Apparatus for Determining Specific Gravity, Method No. 3.

with a towel, re-weighed and immediately introduced into the cylinder which had previously been filled to overflowing with water at room temperature. The displaced water was caught in a weighed beaker and its volume determined by weighing. Calling the weight in air of the dry sample A, and the weight of the displaced water B, the apparent specific gravity is then:

$$\text{Apparent Specific Gravity} = \frac{A}{B}$$

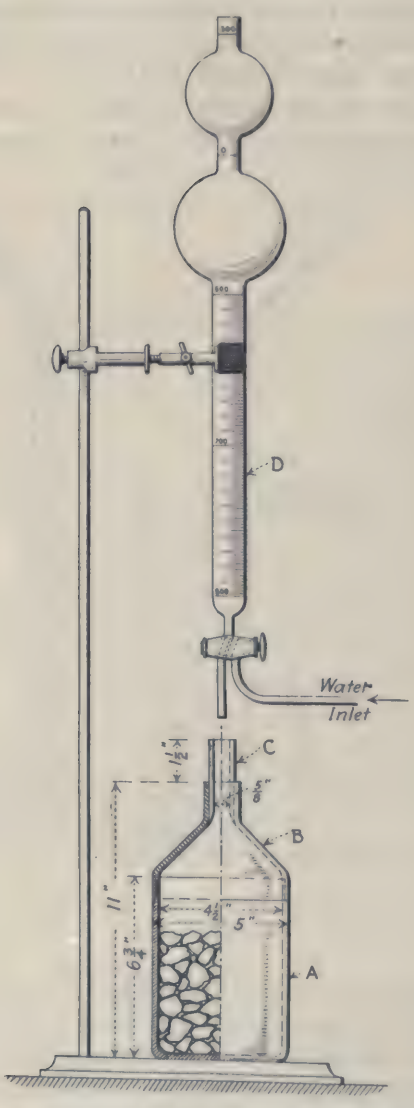

FIG. 3.—Apparatus for Determining Specific Gravity, Method No. 4.

Method No. 4.—The same samples used in method No. 3 were used in this method. The apparatus was designed by the authors and is shown in Fig. 3. It consists of a brass cup *A*, $4\frac{1}{2}$ in. in diameter and $6\frac{3}{4}$ in. high; which is fitted with a conical cap *B*. The sides of the cup are turned down at the top one-half the thickness of the metal, forming a shoulder about 1 in. deep. A similar shoulder cut in the cap provides an absolutely constant volume when the cap is in position. The joint is made water-tight by snapping an ordinary rubber band about $\frac{1}{2}$ in. wide around it. A glass tube *C*, with a single graduation fixes the volume of the cup. A special form of burette *D*, carrying two bulbs of 300 and 600-cc. capacity, and a graduated tube of 200 cc., capable of being read to the nearest cubic centimeter, is most conveniently used for measuring the water. When making a determination, the sample is first treated as under method No. 3. After surface-drying and re-weighing, it is introduced into the brass cup, the cap placed in position and the cup filled with water from the burette up to the graduation on the glass tube *C*. Knowing the volume of the cup, the apparent specific gravity of the material is then calculated by dividing the original weight of the sample by the difference in the volume of the cup and the volume of water necessary to fill it after the introduction of the sample. Calling the weight of the dry sample in air *A*, the volume of the cup filled with water *B*, and the volume of water necessary to fill it after the introduction of the sample *C*, the apparent specific gravity is then:

$$\text{Apparent Specific Gravity} = \frac{A}{B-C}$$

The true specific gravity of the sample obtained by this method may be calculated as follows: Calling the weight in air as before *A*, the volume of the cup *B*, the volume of water required to fill it after introduction of the sample *C*, and the surface-dried weight of the sample after 24 hours immersion *D*, then:

$$\text{True Specific Gravity} = \frac{A}{(B-C)-(D-A)}$$

Method No. 5.—This method has been proposed by Chapman for use in conjunction with method No. 2 when the material is of a non-homogeneous nature. The apparatus used by the authors in making the determinations is shown in Fig. 4,

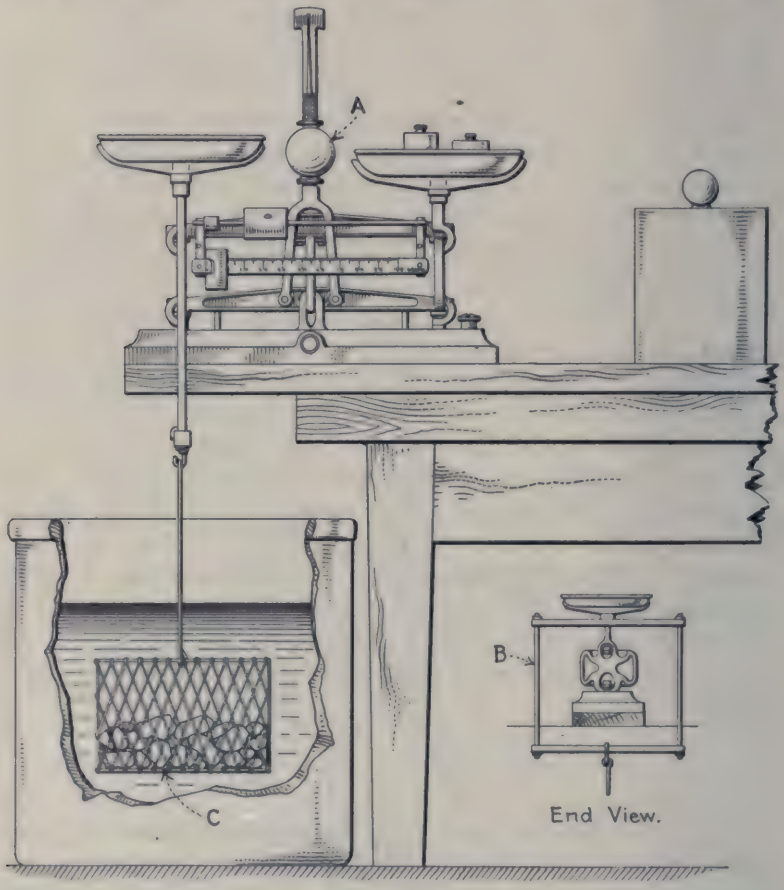

FIG. 4.—Apparatus for Determining Specific Gravity, Method No. 5.

in which *A* is an ordinary torsion balance sensitive to 0.5 g. A light but rigid steel frame *B*, is fastened to the left scale pan. The balance is placed on a table as shown, with the left side projecting over the edge. A wire basket *C* about 5 in. square and

4 in. deep, of ⅛-in. diamond-mesh galvanized wire, is provided with a very light wire and hook for attaching to the frame *B*. In making the determination, the same sample was used as in methods Nos. 3 and 4. After surface drying and re-weighing, the material was placed in the basket which was immediately immersed in water and weighed.

Previous to weighing, the balance was adjusted to zero with the basket immersed in water, so that the weight recorded after placing the sample in the basket might be used directly in computing the specific gravity. On calibrating the modified balance it was found that the strain produced by the weight of the frame and its counterweight caused it to weigh from 1 to 2 g. light. The error caused by this was corrected for all practical purposes by weighing the saturated sample in air on the same balance, giving a constant error, which vanished when the weight in water was subtracted from the weight of the saturated sample in air. The original weight in air was made upon a correct balance. The specific gravity was calculated by dividing the weight in air of the dry sample by the difference in weight of the saturated sample in air and in water, or, calling the weight in air (dry) *A*, the weight in water *B*, and the weight in air (surface-dried) *C*, then:

$$\text{Apparent Specific Gravity} = \frac{A}{C - B}$$

or

$$\text{True Specific Gravity} = \frac{A}{A - B}$$

Method No. 6.—This method was used in the determination of the specific gravity of fine aggregates. The apparatus used is shown in Fig. 5. It is an improved form of the original Le Chatelier flask, designed by the U. S. Bureau of Standards, and is used in testing cement under U. S. Government specifications.[1] The principal advantage over the older type of Le Chatelier flask is a wider tube, ending in a funnel at the top and provided with a ground-glass stopper. The large bulb is also flattened at the bottom as shown in the cut, making the bottle much more stable than the old type. The determination

[1] *Circular No. 33*, Bureau of Standards, p. 27.

of the specific gravity of sand by this method was exactly the same as that specified for cement, except that a 57-g. instead of a 64-g. sample was used, so that the final readings would come within the graduations on the tube. Water was also used

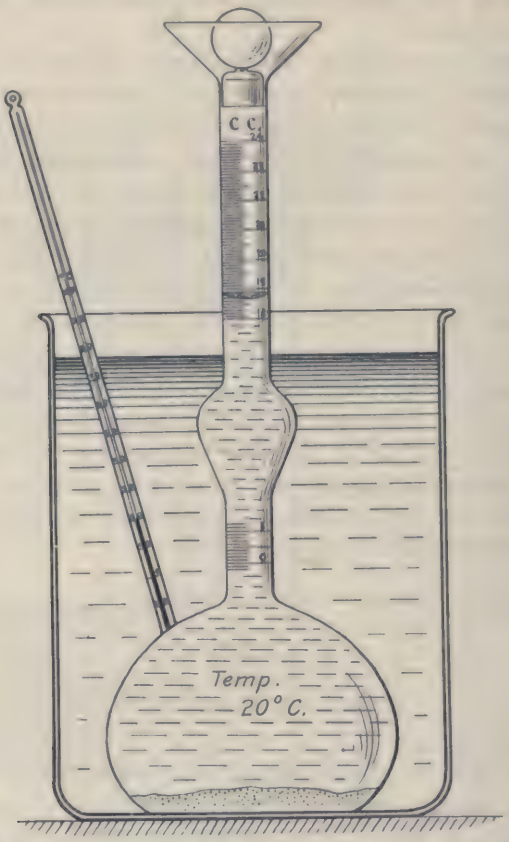

FIG. 5.—Apparatus for Determining Specific Gravity, Method No. 6.

instead of benzine or kerosene. Calling the original weight of the sample A, and the displaced volume as read on the tube B, then:

$$\text{True Specific Gravity} = \frac{A}{B}$$

Method No. 7.—In this method the Jackson apparatus for determining the specific gravity of fine aggregates was used.

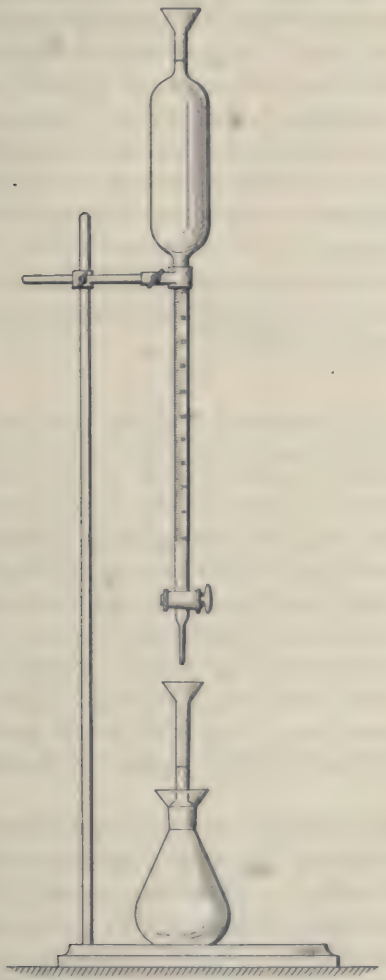

FIG. 6.—Apparatus for Determining Specific Gravity, Method No. 7.

This apparatus is shown in Fig. 6, and consists of an Erlenmeyer flask, fitted with a special form of hollow ground-glass stopper, and having a capacity of 200 cc. up to a graduation on the

stopper tube. A special form of burette with graduations to 0.01 in specific gravity, provided with a bulb of 180-cc. capacity, is used for measuring the water displaced. In making the specific-gravity determination by this method, the burette was first filled with water to the mark above the bulb. Water was then run into the flask until the burette bulb was about half empty. A sample of sand, dried to constant weight and weighing 50 g. ($\pm$0.01 g.), was then slowly introduced into the flask after which the hollow ground-glass stopper was inserted and the flask filled with water up to the graduation on the stopper. The specific gravity was then read directly on the burette. The same precautions relative to temperatures and the exclusion of air bubbles must of course be taken in this method as in the Le Chatelier determination.

SPECIFIC GRAVITY OF COARSE AGGREGATES.

Sixteen samples of crushed rock, three samples of crushed slag and a sample of gravel were chosen for specific-gravity determinations by the first five methods previously described. These samples were chosen with care so as to include as wide a variation in physical properties as would ordinarily be expected in road-building material. The rocks selected were trap, granite, gneiss, limestone and sandstone. Samples for methods Nos. 3, 4 and 5 were prepared as described under method No. 3, and after the tests were completed small specimens of each were carefully selected for the purpose of testing by methods Nos. 1 and 2.

In order to determine the most practicable time of immersion for complete absorption of water, preliminary absorption tests were made, the results of which are shown in Table I. The samples, weighing approximately 1000 g., were all dried to constant weight, cooled, weighed to the nearest 0.5 g. and immediately immersed in water. After ten minutes the samples were removed, the surface wiped dry with a towel, and re-weighed. They were then immersed in water and again weighed at the end of forty minutes. The operation of immersion, drying and weighing was repeated for periods of 1 hour, 24 hours, and in some cases, 48 and 72 hours, until practically constant weight

had been attained. The results of these preliminary tests indicate that in practically all cases the maximum period of saturation is well within 24 hours, and that even in cases where a constant weight has not been absolutely reached at this period, the rate of absorption is so slow after the first 24 hours that the accuracy of results would not be seriously affected thereby.

It was also found that under ordinary conditions it is perfectly practicable to surface-dry a 1000-g. sample of material composed of individual fragments which are 1 in. in diameter or larger in size with the assurance of obtaining check results

TABLE I.—ABSORPTION TESTS OF ROCK.

Sample No.	Description.	Number of Pieces.	Percentage Gain in Weight at End of					
			10 min.	40 min.	1 hr.	24 hr.	48 hr.	72 hr.
8 709	Diabase....	21	0.2	0.2	0.2	0.3	...	...
9 047	Diabase....	23	0.3	0.3	0.3	0.3	...	...
8 786	Diabase....	19	0.0	...	0.1	0.1	...	...
9 229	Limestone..	21	2.0	2.1	2.1	2.6	2.7	2.7
9 347	Limestone..	18	0.1	0.1	0.1	0.2	...	...
9 196	Granite.....	26	0.4	0.4	0.4	0.4	...	...
9 311	Granite.....	20	0.6	0.5	0.5	0.5	...	...
9 192	Granite.....	20	0.3	0.2	0.2	0.3	...	...
9 186	Gneiss.....	20	0.5	0.5	0.5	0.6	...	...
9 935	Limestone..	16	0.7	1.0	1.1	1.1	1.1	1.1
9 935[a]	Limestone..	100	0.4	0.8	1.1	0.5	0.8	1.0
9 957	Limestone..	15	...	...	1.0	1.1	1.0	1.1
9 957[a]	Limestone..	100	...	...	1.1	1.4	1.2	1.6
10 020	Limestone..	15	0.1	0.1	...	0.2	0.2	0.2
10 020[a]	Limestone..	100	0.4	0.3	...	0.4	0.3	0.6

[a] Samples were between ¾ and ½ in. in size. All other samples were between 1½ and 1¼ in.

to the nearest gram or 0.1 per cent of the original weight, and that it is impracticable to surface-dry samples the individual fragments of which are less than ½ in. in size and obtain check results. This is shown by the results obtained on limestone samples Nos. 9935, 9957 and 10,020, for which two sets of results are given. The first set of results was upon samples composed of pieces between 1½ and 1¼ in. in size, and the second set of results, upon samples composed of pieces between ¾ and ½ in. in size. Considerable variation is shown in absorption in the latter case, due undoubtedly to the difficulty of uniformly surface-drying so large a number of pieces. It would, therefore,

seem impracticable to attempt to determine the apparent specific gravity of a mass of particles smaller than ½ in. in size.

The average results of specific-gravity determinations made by methods Nos. 1, 2, 3, 4 and 5, together with the maximum variation obtained for the samples tested, are given in Table II. The general scheme of testing for methods Nos. 3, 4 and 5 was as follows: One determination was made by each of three operators on each sample, consisting of 1000 g. The average of these three determinations is shown in each case,

TABLE II.—APPARENT-SPECIFIC-GRAVITY TESTS OF ROCK.

Sample No.	Apparent Specific Gravity.									
	Method No. 1.		Method No. 2.		Method No. 3.		Method No. 4.		Method No. 5.	
	Average.	Variation.	Average.	Variation.	Average.	Variation.	Average.	Variation.	Average.	Variation.
8 709	3.038	0.032	3.039	0.029	2.952	0.000	2.948	0.008	2.954	0.018
9 047	2.954	0.078	2.955	0.061	2.975	0.010	2.968	0.020	2.969	0.005
8 786	3.033	0.036	3.034	0.040	.....	.....	2.994	0.000	2.996	0.013
9 229	2.555	0.072	2.500	0.109	2.552	0.008	2.439	0.005	2.446	0.010
9 347	2.678	0.008	2.676	0.009	2.658	0.007	2.672	0.007	2.668	0.014
9 885a	2.487	0.280	2.292	.....	2.365	0.028	2.361	0.006	2.362	0.003
9 196	2.658	0.118	2.633	0.032	2.628	0.009	2.636	0.006	2.629	0.024
9 311	2.630	0.015	2.613	0.042	2.673	0.015	2.682	0.008	2.685	0.008
8 823	2.626	0.029	2.632	0.006	2.625	0.011	2.632	0.000	2.642	0.003
8 833b	.....	.....	.....	.....	.....	.....	2.634	0.008	2.639	0.007
9 192	2.688	0.029	2.685	0.035	2.726	0.016	2.739	0.007	2.738	0.007
9 186	2.658	0.046	2.654	0.033	2.634	0.011	2.627	0.007	2.630	0.000
9 021	2.651	0.034	2.644	0.036	2.662	0.023	2.668	0.011	2.666	0.005
9 021b	.....	.....	.....	.....	.....	.....	2.670	0.006	2.672	0.003
8 910	2.488	0.076	2.419	0.009	2.369	0.004	2.377	0.011	2.379	0.005
9 971	2.784	0.029	2.769	0.018	2.786	0.015	2.789	0.009	2.807	0.000
9 971a	.....	.....	.....	.....	2.794	0.000	2.801	0.009	2.804	0.008
9 992	2.707	0.004	2.704	0.006	2.698	0.000	2.706	0.006	2.714	0.013
9 992a	.....	.....	.....	.....	.....	.....	2.703	0.007	2.705	0.015
Aver.....		0.060		0.055		0.011		0.007		0.009

a Sample with rounded edges; all others angular fragments.
b One large piece, weighing approximately 1 kg.
NOTE.—The variation referred to is the difference between the lowest and the highest result.

together with the total variation from the lowest to the highest result. Each determination was independent of the other two, with the exception that the same sample was used by each operator. This makes it possible to determine the probable variation due to the personal equation in making the test. In order to avoid possible error due to abrasion, the original dry weights were checked from time to time.

At the conclusion of these tests, three small fragments of from ½ to 1 in. in size were taken from each of the large samples and tested according to methods Nos. 1 and 2, these frag-

ments representing in each case as nearly as possible the average of the large sample. After drying these small samples to constant weight, one determination by method No. 1 and one by method No. 2 was made on each specimen by each of two operators, making a total of six determinations by each method on each sample. By this means variations resulting from the personal equation of the operators and also variations resulting from differences in the individual specimens were obtained. A detailed comparison of methods Nos. 1 and 2 is given in Table III.

Referring first to Table II, it may be seen that in practically every instance three operators checked by methods Nos. 4 and 5 to within one unit in the last of three significant figures. In only one case in method No. 4 and two cases in method No. 5 were the maximum variations over 0.015 out of a total of 19 samples tested. In method No. 3 a variation of over 0.015 was obtained in 3 samples out of a total of 15 tested. So far as the average determinations are concerned, methods Nos. 3, 4 and 5 give essentially identical results to the third significant figure.

With regard to methods Nos. 1 and 2 in comparison with methods Nos. 3, 4 and 5, it will be noted that in many instances the average specific gravity differs by a number of units in the second decimal place or in the third significant figure, although in certain instances, presumably where the material is of a homogeneous nature, the results are almost identical. The maximum variation between the individual specimens by either of these methods is, however, shown to be quite large, amounting in the case of sample No. 9888 to 0.289 for method No. 1 and 0.355 for method No. 2. It is apparent, therefore, that these methods are not so generally applicable as methods Nos. 3, 4 and 5.

It will be noted that in certain cases samples composed of fragments with rounded edges were tested in comparison with angular fragments of the same material. The results in both cases are so nearly the same that it would appear immaterial whether rounded or angular fragments were used for this test. In two instances a single large fragment of material was subjected to test. While these tests are not conclusive it would appear, nevertheless, that there is practically a negligible dif-

ference in the apparent specific gravity of a single fragment of material weighing 1 kg. and the average specific gravity of twenty fragments of the same material, whose total weight is 1 kg.

TABLE III.—COMPARISON OF METHODS NOS. 1 AND 2.

Sample No.	Method No.	Apparent Specific Gravity.						
		Specimen No. 1.		Specimen No. 2.		Specimen No. 3.		Average.
		Operator No. 1.	Operator No. 2.	Operator No. 1.	Operator No. 2.	Operator No. 1.	Operator No. 2.	
8 709	1	3.015	3.044	3.049	3.044	3.022	3.019	3.038
	2	3.050	3.051	3.047	3.049	3.022	3.027	3.039
9 047	1	2.955	2.952	2.920	2.933	2.975	2.998	2.954
	2	2.935	2.945	2.940	2.933	2.990	2.994	2.955
8 786	1	3.040	3.036	3.049	3.040	3.019	3.013	3.033
	2	3.039	3.052	3.031	3.043	3.012	3.026	3.034
9 229	1	2.564	2.570	2.535	2.513	2.585	2.561	2.555
	2	2.465	2.468	2.468	2.476	2.574	2.549	2.500
9 347	1	2.673	2.669	2.678	2.678	2.681	.....	2.678
	2	2.672	2.672	2.678	2.674	2.681	2.677	2.676
9 288	1	2.552	2.590	2.420	2.572	2.301	.....	2.487
	2	2.525	2.517	2.170	2.182	2.179	2.180	2.292
9 196	1	2.655	2.654	2.630	2.748	2.631	2.630	2.658
	2	2.632	2.640	2.630	2.634	2.617	2.608	2.628
9 311	1	2.621	2.628	2.636	2.636	2.625	2.633	2.630
	2	2.582	2.593	2.592	2.600	2.618	2.624	2.613
8 851	1	2.609	2.638	2.617	.....	2.632	2.634	2.626
	2	2.635	2.635	2.630	2.632	2.632	2.629	2.632
9 192	1	2.691	2.699	2.683	2.692	2.670	2.690	2.688
	2	2.689	2.697	2.683	2.694	2.662	2.682	2.685
9 186	1	2.650	2.629	2.675	2.654	2.668	2.669	2.658
	2	2.632	2.638	2.662	2.660	2.665	2.664	2.654
9 021	1	2.635	2.652	2.635	2.662	2.655	2.669	2.651
	2	2.637	2.645	2.628	2.645	2.642	2.664	2.644
8 910	1	2.452	2.528	2.497	2.491	2.478	2.483	2.488
	2	2.416	2.423	2.416	2.417	2.416	2.425	2.419
9 971	1	2.779	2.772	2.775	2.782	2.801	2.792	2.784
	2	2.763	2.764	2.762	2.772	2.775	2.780	2.769

Referring now to Table III, it will be seen that results obtained by different operators on the same individual specimen are in the majority of cases within the practical limit of 1 in the third significant figure for method No. 2, while in method No. 1 considerably wider variations are found to exist in certain instances. Method No. 2 is therefore somewhat preferable to method No. 1 for homogeneous materials. This is probably due

to errors caused in method No. 1 by the absorption of water during weighing.

In Table IV a comparison of methods Nos. 1, 2, 3, 4 and 5 is given for three samples of slag. Here again the superiority of methods Nos. 3, 4 and 5 over methods Nos. 1 and 2 for non-homogeneous material is clearly evident, with method No. 5 in general showing the least variation on the same sample tested by different operators. It should be noted that sample No. 9750 was, to all appearances, a fairly homogeneous product, while sample No. 9954 was quite porous.

As a final means of comparing apparent-specific-gravity determinations made upon single small specimens and 1000-g. samples, four aggregates were selected for test by methods Nos. 2

TABLE IV.—APPARENT-SPECIFIC-GRAVITY TESTS OF SLAG.

Sample No.	Apparent Specific Gravity.									
	Method No. 1.		Method No. 2.		Method No. 3.		Method No. 4.		Method No. 5.	
	Average.	Variation.	Average.	Variation.	Average.	Variation.	Average.	Variation.	Average.	Variation.
9 750	3.457	0.015	3.459	0.013	3.573	.....	3.565	0.012	3.570	0.007
9 953	.....	.....	.....	.....	3.846	.....	3.836	0.027	3.839	0.012
9 954	2.689	0.266	2.474	0.614	2.206	.....	2.200	0.010	2.203	0.015

NOTE.—The variation referred to is the difference between the lowest and the highest result.

and 5. Two of these samples, a gravel and a dolomite, were taken because they were obviously non-homogeneous. A limestone and sandstone were also selected because they were apparently homogeneous and showed a relatively high absorption. From three to five samples of each were tested by both methods. The results are given in Table V.

As was expected, an exceedingly wide variation for the non-homogeneous products was given by method No. 2. While the variation was much smaller by method No. 5, in the case of the gravel, it was sufficiently wide to indicate the necessity of averaging a number of determinations upon very non-homogeneous aggregates where accurate results are desired. Even in the material which was apparently homogeneous, method No. 2 is shown to be far less accurate than method No. 5.

Incidental to the determinations of apparent specific gravity it was thought of interest to ascertain the effect of absorption as applied to the calculation of true specific gravity. That

TABLE V.—COMPARISON OF METHODS NOS. 2 AND 5 FOR DETERMINING THE APPARENT SPECIFIC GRAVITY OF NON-HOMOGENEOUS AGGREGATES.

Average Absorption in 24 Hours, percent.	Sample No.	Description.	Method No.	Apparent Specific Gravity.						Variation.
				Specimen No. 1.	Specimen No. 2.	Specimen No. 3.	Specimen No. 4.	Specimen No. 5.	Average.	
0.5	9 003	Gravel.....	2	2.637	2.643	2.876	2.417	2.654	2.645	0.459
			5	2.572	2.597	2.575	2.552	2.589	2.577	0.045
0.1	9 935	Dolomite...	2	2.780	2.798	2.589	2.573	.....	2.685	0.225
			5	2.660	2.671	2.650	.....	.....	2.660	0.021
1.0	10 026	Limestone..	2	2.730	2.633	2.631	2.735	.....	2.682	0.099
			5	2.698	2.678	2.662	.....	.....	2.679	0.036
3.7	10 036	Sandstone..	2	2.400	2.379	2.379	2.361	.....	2.380	0.039
			5	2.392	2.410	2.387	.....	.....	2.396	0.023

NOTE.—The variation referred to is the difference between the lowest and the highest result.

TABLE VI.—COMPARISON OF APPARENT AND TRUE SPECIFIC GRAVITIES BY METHODS NOS. 4 AND 5.

Sample No.	Absorption in 24 Hours, per cent.	Specific Gravity.					
		Method No. 4.			Method No. 5.		
		True.	Apparent.	Difference.	True.	Apparent.	Difference.
8 709	0.3	2.971	2.948	0.023	2.971	2.954	0.017
9 047	0.3	2.990	2.968	0.022	2.991	2.969	0.022
8 786	0.1	3.002	2.994	0.008	3.008	2.996	0.002
9 229	2.7	2.608	2.439	0.169	2.608	2.446	0.162
9 347	0.2	2.699	2.672	0.027	2.700	2.668	0.032
9 198	0.4	2.681	2.636	0.045	2.675	2.629	0.046
9 311	0.5	2.725	2.682	0.043	2.722	2.685	0.037
8 833	0.2	2.641	2.632	0.009	2.659	2.642	0.017
9 192	0.3	2.764	2.739	0.025	2.770	2.738	0.032
9 186	0.6	2.671	2.627	0.044	2.678	2.630	0.048
9 021	0.5	2.702	2.668	0.034	2.702	2.666	0.036
8 910	4.6	2.690	2.377	0.313	3.700	2.379	0.321
9 971	0.5	2.829	2.789	0.040	2.845	2.807	0.038
9 992	0.2	2.716	2.706	0.010	2.715	2.714	0.001

the difference between apparent and true specific gravity for some rocks may be quite pronounced is shown in Table VI, which gives the comparisons by methods Nos. 4 and 5 of all

rocks shown in Table II. Here it will be noted that in one case, that of sample No. 8910, the true specific gravity exceeded the apparent by over 3 in the second significant figure. This amounts to an actual variation of over 12 per cent.

SPECIFIC GRAVITY OF FINE AGGREGATES.

For the determination of the specific gravity of fine aggregates, three samples of sand were selected, one being a coarse concrete sand, another a fine sheet-asphalt sand and the third, standard Ottawa sand such as is used in cement testing. Tests were run by means of methods Nos. 6 and 7 and also by method No. 4 which was apparently the best adapted for fine aggregates of any of the methods employing approximately 1000-g. samples

TABLE VII.—TRUE SPECIFIC GRAVITY OF SAND.

Sample No.	Character of Sand.	True Specific Gravity.					
		Method No. 4.		Method No. 6.		Method No. 7.	
		Average.	Variation.	Average.	Variation.	Average.	Variation.
9 984	Coarse Concrete.....	2.654	0.020	2.665	0.003	2.669	0.005
9 934	Asphalt.....	2.646	0.017	2.655	0.013	2.653	0.008
....	Ottawa.....	2.646	0.025	2.664	0.008	2.652	0.016

NOTE.—The variation referred to is the difference between the lowest and the highest result.

for test. The results given in Table VII indicate that method No. 4 is not well suited to the determination of the specific gravity of fine aggregates, due, probably, to the difficulty of freeing so large a sample from air. There is apparently but little choice between methods Nos. 6 and 7, although method No. 6 is believed to be more satisfactory from the standpoint of time consumed. With one exception, the maximum variation obtained upon different samples of the same material by either method No. 6 or No. 7 made a difference of 1 in the third significant figure. The average specific gravities obtained by methods Nos. 6 and 7 check within 1 in the third significant figure.

TIME REQUIRED FOR MAKING TESTS.

The time required to make determinations of specific gravities by the various methods described, exclusive of the time utilized in preparing the test specimen and, in the case of methods Nos. 3, 4 and 5 of the 24-hour period of absorption, is as follows: Method No. 1, 15 minutes; No. 2, 40 minutes; Nos. 3, 4 and 5, each 5 minutes; No. 6, 30 minutes; and No. 7, 45 minutes. It will be seen that no difference exists between methods Nos. 3, 4 and 5 so far as time is concerned. Method No. 6, however, is considerably shorter than method No. 7, because of the time required to dry out the flask of the Jackson specific-gravity apparatus after making one determination, in order to prepare it for a second determination.

CONCLUSIONS.

As the results of the investigations above described, the following conclusions have been drawn:

1. In the case of rock and slag an appreciable variation may exist between apparent specific gravity and true specific gravity, depending upon the absorption of the material.

2. The displacement method formerly used by the Office of Public Roads and Rural Engineering is not as satisfactory as the Chapman method for determining the apparent specific gravity of single specimens of rock, slag or gravel, unless the difference between apparent specific gravity and true specific gravity is negligible.

3. The Chapman method is satisfactory for determining the apparent specific gravity of single specimens of rock, slag, etc., but is not a safe one to employ in determining the specific gravity of non-homogeneous or apparently homogeneous aggregates, even when the average results of three apparently representative specimens are taken.

4. In the case of non-homogeneous aggregates consisting of fragments of not less than $\frac{1}{2}$-in. in diameter, either the Goldbeck method, the Chapman wire-basket method or the Hubbard-Jackson method are satisfactory, and can ordinarily be depended upon to give check results by different operators working upon the same sample to within 1 in the third significant figure.

5. It is impracticable by any of the methods studied to determine the apparent specific gravity of samples composed of fragments smaller than $\frac{1}{2}$ in.

6. When determining the specific gravity of extremely non-homogeneous aggregates it is recommended that an average of not less than three tests made upon different samples by the Goldbeck, Hubbard-Jackson or Chapman wire-basket method, be reported.

7. It has been found that the Bureau of Standards' modification of the Le Chatelier apparatus is more convenient and rapid for determining the specific gravity of aggregates, the individual fragments of which are less than $\frac{1}{4}$ in. in diameter, than is the Jackson apparatus.

8. When it is desired to obtain as nearly as possible the apparent specific gravity of aggregates consisting of a mixture of coarse and fine particles, it would appear advisable to separate a weighed sample of the material by means of a $\frac{1}{2}$-in. screen and make an apparent-specific-gravity determination upon not less than 1000 g. of the coarse fraction, and a true-specific-gravity determination upon not less than 50 g. of the finer fraction. The specific gravity of the whole may then be calculated from the following formula, where W equals the percentage by weight of coarse aggregate, W' the percentage by weight of fine aggregate, and G and G' their respective specific gravities:

$$\text{Specific Gravity of Aggregate} = \frac{100}{W/G + W'/G'}.$$

DISCUSSION.

Mr. Lasier. MR. E. L. LASIER.—Mention is made in the paper of the difficulty of obtaining the true specific gravity of a mass of material. This is, of course, due in part to the near inability of obtaining a total absorption of water in all pores and air spaces; that is, it would be difficult to secure a constant weight for samples immersed in water.

I should like to ask the authors if they made any determinations based on samples boiled in water.

Mr. Jackson. MR. F. H. JACKSON, JR.—The whole idea in making the absorption tests, so far as the work of this paper is concerned, was simply to reach a point where absorption was practically complete, so that the apparent specific gravity could be obtained. The only object in obtaining results on true specific gravity was to show the effect of absorption on the determinations. I think there is no question but that the apparent specific gravity is what we want, especially in practical work, and not the true specific gravity. I do not see any necessity for attempting the accurate determination of the true specific gravity of coarse aggregates.

THE RELATION BETWEEN YIELD POINT AND PROPORTIONAL LIMIT IN VARIOUS GRADES OF STEEL.

The discussion of this subject was formally opened by Mr. J. E. Howard, Mr. T. D. Lynch, and Mr. H. F. Moore and Mr. F. B. Seely. The papers by these authors, with accompanying summaries, are followed by the General Discussion of this subject.

PAPER NO. 1.

BY J. E. HOWARD.

SUMMARY.

Reference is made to the modulus of elasticity as the fundamental feature on which it is convenient to judge of the elastic limit of steel. With a constant value for the modulus of elasticity any departure of the stress-strain curve from a straight line signifies the introduction of a permanent set and establishes the value of the elastic limit. The behavior of steel is such that the terms elastic limit and proportional limit are synonymous. The former is preferred on account of its definite meaning, it being conceivable that the progressive development of permanent sets might take place in such degree that the proportional limit might include permanent sets of magnitude.

The term "yield point" is considered to be without definite meaning and does not attach to any particular part of the stress-strain curve, but is generally taken to mean a value somewhat above the elastic limit. It is not considered a desirable nor useful term to employ.

It is pointed out that the presence of initial strains causes the introduction of local sets before the elastic limit of the steel as a whole is reached. Also, that suitable facilities are not always available in testing equipment whereby the elastic limit may be satisfactorily determined. Further, that indifferent manipulation is a source of error in failing to properly adjust test specimens so that they may be pulled axially.

THE RELATION BETWEEN YIELD POINT AND PROPORTIONAL LIMIT IN VARIOUS GRADES OF STEEL.

By J. E. Howard.

Use of Modulus of Elasticity in Determination of Elastic Limit.—In the testing of steel the property first to be observed is its elasticity. It is extensible and compressible, having the ability to recover its shape provided the strains are small. Its rate of extension or compression, that is, its modulus of elasticity, is the first factor to be measured, and this constitutes the fundamental feature on which it is convenient to judge of the elastic limit of the metal.

Provided the modulus of elasticity has a constant value under different loads, which appears to be true in the case of steel that has not been overstrained, it follows that the stress-strain curve will be represented by a straight line, and any departure therefrom will signalize the introduction of a permanent set. The point of departure of the curve from a straight line will be its proportional limit, or preferably it may be designated as its elastic limit, the term under which it has so long been known.

This brief introductory statement of the case virtually comprises all that appears necessary to say on the subject, and the paper might be summarily brought to a close so far as it relates to the definition of what constitutes the elastic limit of steel.

If the composition of steels were to be extended over so wide a range that different values in the modulus of elasticity prevailed, such a circumstance would not affect the case in judging of the limit at which a permanent set was first developed in any given grade. Engineering practice, it is known, does not recognize different values in the modulus of elasticity covering the usual range in composition of structural steels, nor does it assume that the modulus possesses a variable value, depending

upon the intensity of loading. Unless a constant value prevailed, structural engineering would be in a state of utter confusion.

The term elastic limit inherently carries with it a definite meaning. It conveys the idea of a limit beyond which the resilience of the metal is incomplete; that permanent sets are introduced when the steel is strained beyond this limit. The expression needs no qualifying adjective to explain or restrict its meaning. The term "limit of perfect elasticity" can only be synonymous with the expression elastic limit.

Elastic Limit and Proportional Limit the Same.—The behavior of steel is generally such that the elastic limits and proportional limits coincide; nevertheless, it would not be inconsistent with the meaning which might attach to the latter expression were the extensions or compressions of the metal to include both elastic strains and progressive permanent sets. A restricted meaning should properly be placed on the term proportional limit to exclude permanent sets, without which it presents no barrier against the presence of permanent sets of any magnitude.

Preference may very properly be given a term, the use of which in itself conveys the meaning for which it is employed, and for this reason the term elastic limit commends itself.

With a known value of the modulus of elasticity the strain under any given load may be anticipated and followed during the test of the steel, and without release of load, or interruption to the progress of the test the elastic limit may be recognized and the amount of permanent set determined when the stresses shall have exceeded the elastic limit. A change occurs in the value of the modulus of elasticity in overstrained steel, noticeable after the development of decided permanent sets. The value is then variable, having a diminished value as loads are advanced.

Elastic Limit Sharply Defined in Mild Steels.—For loads below the elastic limit there is an immediate response upon application of the straining forces, the strains promptly developing in full degree. For stresses above the elastic limit, particularly in mild steels, the full development of sets takes place

more or less sluggishly. In mild steels, jogs not infrequently appear just after the elastic limit has been reached during which period strain proceeds under diminished stresses. This is suggestive of a zone of structural instability in the vicinity of the elastic limit. At certain temperatures, above atmospheric, several jogs make their appearance in the same test specimen.

In steels of such grades there can be very little difficulty in acquiring a close approximate knowledge of the elastic limit under ordinary conditions of testing. If the test specimen has the mill scale upon it, the brittle character of the scale may serve as an index of the elastic limit having been exceeded, when it flakes off.

In the harder steels the introduction of permanent sets takes place gradually, without abrupt change in the direction of the stress-strain curve. Jogs are absent, that is, there is no extension of the metal under loads lower than it had previously sustained. With increase in hardness, by composition or by heating and quenching, the steel tends toward maintaining a straight line in the stress-strain curve until there is a close approach to its ultimate strength.

Working the steel at temperatures below the usual rolling or forging heats modifies the shape of the stress-strain curve. If the metal is extended by tensile stresses while at a blue heat, in the zone of 400 to 600° F., a decided increase in both elastic limit and tensile strength may result. If the straining is done when the steel is cold, quite naturally the elastic limit is raised, in the direction of the overstraining force. Permanent extension which is exhausted under a given load is not renewed until a higher stress is reached.

Overstraining Test Specimen before Determining Elastic Limit Undesirable.—Recommendations are occasionally made to initially overstrain the steel and then retest for its elastic limit. Such pre-treatment might be carried to any extent, with corresponding effect upon the elastic limit in the direction prestrained. Obviously, such treatment does not commend itself. Straining cold, however efficacious it may be in raising the elastic limit to any desired value (in the case of music wire the cold drawing results in phenomenal elastic limit and tensile

strength), nevertheless has a detrimental effect on the value of the opposite elastic limit. Earlier tests have shown that the total range in elastic movement is greater in the primitive state of the hot-worked steel before either elastic limit of tension or compression has been exceeded, than in its subsequent state after an overload has been applied in either direction. That is, the loss in the reverse direction is greater than the gain in the elastic limit in the direction of the overstraining load. A proposed method of testing which involves such results, or has such tendencies, does not appear to be a desirable one.

It must be assumed that the object of testing is to determine the pre-existing physical properties, and not to introduce new combinations by the process or method of testing. Of course, tests may be conducted for the purpose of ascertaining the effects of different kinds of treatment of the steel, but not to accredit values to the steel which are due to the manner of making the tests.

It is a matter of ordinary occurrence to find, in the testing of steels, that permanent sets of small magnitude make their appearance at an early stage, and before there is reason for believing that the metal as a whole is overstrained; that in some part of its volume sets are developed which have a local character, while the elastic limit of the greater part of the material has not at the time been reached.

Initial Strains in Steel Obscure Elastic Limit.—One of the common causes which leads to the development of local sets is the presence of initial or internal strains in the steel. Internal strains not infrequently exist in steel which is not under the influence of external loads. The metal is in a state of equilibrium, therefore the initial strains must be of two kinds, namely, those of tension and those of compression.

The relative intensities of these opposing strains may vary in different cases, and it follows that the volumes of metal in tension and in compression will be unequal. Under these conditions permanent sets will be developed locally, earlier or later, according to whether the steel is loaded by tensile or by compressive stresses. The initial strains may have values practically coinciding with the elastic limit of the metal, and

would then be expected to display sets under the earliest applied loads, increasing with each increment.

Initial strains are readily introduced by differences in the normal rate of cooling of one part over another, whether in the ingot or in rolled or forged shapes; also by accelerated cooling in air or other quenching media. Initial strains may be introduced by mechanical means. It is difficult to work steel without introducing initial strains of greater or less magnitude. The operations of shearing and punching introduce initial strains, and the machining of finished shapes must be carefully done to avoid causing such strains. Bending stresses, with the metal either hot or cold, are prolific causes in the introduction of initial strains.

The influence of initial strains in modifying the shape of the stress-strain curve is easily recognized. They cause the gradual and progressive development of local permanent sets and tend to obscure the elastic limit of the metal or detract from its sharp definition. Certain high physical properties are attained only in association with some of these causes.

The partial or complete elimination of initial strains may be accomplished by annealing the metal, and apparently by annealing only. By mechanical means internal strains may be modified, and differently disposed in the cross-section of the member, and some portions brought to a neutral state, but the entire mass does not admit of being relieved in this manner. It is improbable that the entire effacement of initial strains is accomplished in ordinary annealing operations.

So far as may be judged, initial strains, the primitive values of which coincide with the elastic limit of the cold steel, are progressively dislodged as the temperature of the metal is raised. The lowering of the elastic limit as higher temperatures are reached appears to be the explanation of this. Initial strains can hardly exist having higher values than the strains corresponding to the elastic limit of the steel at any given temperature, hence residual strains may be taken as an index of the elastic limit of the steel at the annealing heats, the time element being a factor.

When cold, the initial strains should not affect the shape

of the stress-strain curve below that extension of the metal at which the combined internal strains and the strains due to the external force attain a maximum, equal to the elastic limit of the steel in its cold state. This statement of the case explains why permanent sets may progressively appear in greater degree as the normal stress representing the elastic limit is approached.

Unsatisfactory Manipulation of Test a Source of Danger.—Another cause for the introduction of permanent sets, of a premature character, attaches to the manipulation of the test, a source of error more common than should exist. Test specimens which are not axially pulled, progressively acquire sets along the overstrained elements. Results are currently reported which show great variations in the extensions on different sides of the test specimen, even negative extensions appearing in the records.

Some Testing Apparatus not Adapted for Determination of Elastic Limit.—Some testing machines are not provided with suitable holders for axially straining the test specimens. Indifferent manipulation accounts for eccentric pulling in other cases. Erratic behavior does not characterize steel when its modulus of elasticity is the only factor which is called into play. Extensometer readings, under early increments of loads, serve to show the satisfactory progress of the test or otherwise as the case may be. Readings on different sides of the test specimen, while they promptly show whether the pull is an axial one, or eccentric, are hardly necessary for the purpose. The modulus of elasticity in steel has so nearly a constant value that the appropriate extensions for given loads may be taken as indexical that the test is being properly conducted. The adaptation of extensometer devices for the purpose of indicating the mean strains for test specimens under eccentric pulling loads, do not commend themselves as substitutes for the proper treatment of the metal.

Speed of Testing not Objectionable with Suitable Testing Equipment.—Concerning the rate of speed of testing, with suitable apparatus, rapid work is of some advantage in the determination of the elastic limit in continuous testing. The influence of time, in respect to rapidity of loading, has no practical

effect on the display of elastic extensions. The strains which are displayed by the steel correspond to the stresses, however rapid the application or alternation of loads may be. This has been shown in the case of repeated alternate stresses, and there is no reason for doubting the same to be true for direct loads of tension or compression. The conditions attending ordinary testing, however, are such that very rapid extensiion are difficult to follow. This limitation pertains to the testngs apparatus rather than to the behavior of the metal.

The rate of testing is quite a different matter when the stresses exceed the elastic limit and the resistance of the steel against viscous flow is encountered. When the elastic limit, particularly in ductile metal, has been passed, the full display of permanent extension is sluggishly developed. The full development of permanent sets under each successive increment of load necessarily affects the shape of the stress-strain curve; a matter not directly under consideration at this time.

Observations to be Made on Test Specimen, not on Testing Machine.—If observations are directed to the behavior of the testing machine itself, and not made on the test specimen, then rapid testing may readily be found undesirable. The results on testing machines of the ordinary commercial type do not always admit of being referred to in a critical discussion of the elastic limit. The means employed in testing must be adequate for the determination of the property under consideration, and the behavior of the steel is in some cases more sensitive than the apparatus used in its examination. If adequate apparatus is employed to judge of the introduction of permanent sets, it is not felt that the rate of loading will materially modify the recognition and definition of the elastic limit.

Vagueness of So-Called Yield Point.—It has not been undertaken in the present remarks to point out a relation between the elastic limit and any so-called yield point. The term "yield point" does not appear to have any fixed meaning, and unless it is made to apply to some definite feature it would be held impractical to establish a relation between a definite and an indefinite object.

There seems to be no feature in the behavior of steel which

calls for the use of this term, or to which it could properly apply. The term is essentially vague and uncertain, and well calculated to create confusion. Steel, as in the case of all substances, yields with the first increment of load, after which it becomes a matter of degree, changing its rate, when permanent extensions are ushered in. It is tacitly accepted that the term yield point, when used, will mean a value above the elastic limit. It is an approximation to the elastic limit, but having been given a name it purports to have a definite meaning, and should define some useful feature or limit in the use of steel. The most that can be said about it is that a structural member loaded up to its yield point will suffer a certain amount of permanent strain.

If circumstances do not admit of the careful determination of the elastic limit, it would convey all the useful information justifiable to report to designate the value given as the approximate elastic limit. It is of course possible to pre-establish a certain limit in the amount of permanent set to be developed and to give that limit a name, but it is not clear that any further advantage would result over its use than would attend calling it the approximate elastic limit. Experience has shown that a very critical determination of the elastic limit is essential in order to judge of the durability of steel which is exposed to repeated alternate stresses, that possibly such critical determinations even may not go far enough to meet the requirements of the case, and that the treatment of the steel should be considered in conjunction with its elastic limit. Service requirements which demand the careful determination of the elastic limit are frequently of such a nature that approximate determinations may have no value or may be misleading and harmful. Such a factor as the elastic limit purports to be should possess as great reliability as possible, and when satisfactory determination is not feasible it is believed that the report of the test should indicate its approximate character.

In closing this paper, introductory to a topical discussion on the subject matter assigned, and set forth by its title, it is hardly necessary to add that a relation has not been mentioned between yield point and proportional limit, since, according to

the views of the writer, one of these terms is vague and without definite meaning while the other is held to be synonymous with the expression elastic limit, the expression which is regarded as preferable to use.

THE RELATION BETWEEN YIELD POINT AND PROPORTIONAL LIMIT IN VARIOUS GRADES OF STEEL.

PAPER NO. 2.

BY T. D. LYNCH.

SUMMARY

The examination of many representative tests of steel leads us to conclude that steel may be divided into two general types, the one having a relatively large ratio between the yield point and the proportional limit, the other having a small corresponding ratio. This relation is being studied by engineers who must specify material for high stresses, and they are beginning to realize that the proportional limit is the real value to be used.

Typical curves are introduced illustrating the difference between these two types and showing the effects produced by cooling slowly from annealing temperatures, cold working, heat treatment, etc.

For convenience the curves for the two types are referred to as "gradual" and "abrupt," respectively.

The gradual curve presents a long and gradual curvature between the proportional limit and the yield point and is shown in tests from untreated steel castings, heat-treated steel forgings, and heavy plates finished hot at the rolls.

The abrupt curve presents a short and relatively sharp curvature between the proportional limit and yield point and is shown in tests from steel castings subjected to cold work or heat treatment, rounds finished relatively cold at the mill and from various grades of heat-treated steel.

The advancement made in the heat treatments of steel during recent years has been so rapid and so important that

engineers may well study the type of curve to be found in different grades of steel subjected to different heat treatments.

This study should include the correlation of the chemistry, manufacture, heat treatment and the resultant physical characteristics shown in actual curves. A knowledge of the material to be used and the stresses to be met in service should enable engineers to so proportion their design and select the material and treatment, that the load applied will always fall below the proportional limit of the material and thereby place in service only that material which will be safe and at the same time economical.

THE RELATION BETWEEN YIELD POINT AND PROPORTIONAL LIMIT IN VARIOUS GRADES OF STEEL.

By T. D. Lynch.

The yield point is defined by Committee E-1 on Methods of Testing as "the load per unit of original area at which a marked increase in the deformation of the specimen occurs without increase of load;" the proportional limit is defined by the same committee as "the load per unit of original cross-section at which the deformations cease to be directly proportional to the loads."

The relation between the yield point and the proportional limit of steel may be divided into two general types, the one having a relatively large ratio between the yield point and the proportional limit, the other having a small corresponding ratio. This subject is of growing interest and is being given a great deal of constructive thought by those who are responsible for materials in design, for the reason that many engineers make use of the yield point in their calculations, while in fact the proportional limit is the important value to know and to use when designing.

The writer has had the pleasure of presenting two papers[1] before this Society on this general subject, in which he described some of the work done at the laboratories of the Westinghouse Electric and Manufacturing Co. and the studies given to the determination of the action of materials under stress. These papers should be considered introductory to this discussion, so that the details given in them need not be repeated here.

The yield point of steel, determined by the drop of beam, is not a very reliable quantity at best, and much depends upon the sensitiveness of the testing machine and the skill of the operator, and when determined by the dividers it is equally unreliable, unless a definite elongation is arbitrarily pre-determined to be the proper amount for the steel under test. A careful study of

[1] "The Use of the Extensometer in Commercial Work," *Proceedings*, Am. Soc. Test. Mats., Vol. VIII, p. 640 (1908); "Elastic Limit," *ibid.*, Vol. XV, Part II, p. 415 (1915).

the curves of many tests of steel indicates that an elongation of 0.5 per cent is a reasonable limit for steel having a yield point not in excess of 100,000 lb. per sq. in. While this figure is purely arbitrary, it has been found that the average operator, taking the yield point by means of dividers, will take his reading when the specimen shows an elongation of about 0.01 in. For a specimen of 2-in. gage length, this is an elongation of 0.5 per cent. In cases where the yield point is determined by the drop of beam—if there is a decided yield point—a reading at an elongation of 0.5 per cent will be found to lie well within the range of the "drop of beam."

In cases where there is no decided yield point, the drop-of-beam method does not show anything of value. Therefore,

TABLE I.—CHEMICAL COMPOSITION OF STEELS.

Material.	Fig. No.	Carbon, per cent.	Manganese, per cent.	Phosphorus, per cent.	Sulfur, per cent.	Silicon, per cent.	Nickel, per cent.
Steel Casting.....	1	0.29	0.69	0.048	0.041	0.28	....
Steel Casting.....	2	0.27	0.73	0.052	0.039	0.28	....
Steel Forging.....	3	0.475	....	.....	.....	....	....
Rivet Steel.....	4	0.15	0.41	0.006	0.065	Trace	....
Steel Plate	6	0.28	....	.....	.....	....	....
Nickel Steel.....	7	0.25	0.56	0.024	0.022	0.14	3.25

some definite amount of deformation must be assumed to indicate the yield point. For the purposes of this paper, in order to standardize results, it is assumed that an elongation of 0.5 per cent while the stress is applied and the testing machine running, indicates that the yield point has been reached.

The proportional limit can, however, be determined quite accurately and the results of tests made from the same specimen of steel by different operators, or by the same operator at different times, should check very closely. A few curves plotted from actual laboratory tests will serve to illustrate the characteristics of a few different types and conditions of steel, and the relation between their respective yield points and proportional limits. All tests were made on standard 0.505-in. diameter test specimens. The chemical compositions of these steels are given in Table I; the physical properties are given in Table II.

Fig. 1 shows a set of curves plotted from the test records of a specimen cut from a sand-cast coupon, not having had any subsequent annealing or heat treatment. A test specimen was cut from the coupon, placed in the testing machine and sufficient

TABLE II.—RESULTS OF TENSION TESTS.

Material and Fig. No.	Tensile Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Proportional Limit, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	Ratio of Yield Point to Proportional Limit, per cent.	Brinell Hardness Number.	Remarks.
STEEL CASTING.								
Fig. 1 *A*.....	.....	36 000	25 000	0.635	....	144.0	168.0	As cast.
B.....	.....	43 100	38 000	0.635	....	113.4	.....	Drawn at 100° C. for 10 min.
C.....	.....	48 500	43 000	0.635	....	112.8	.....	Drawn at 100° C. for 10 min.
D.....	.....	53 500	48 000	0.635	....	111.5	.....	Drawn at 100° C. for 10 min.
E.....	.....	56 900	51 500	0.635	....	111.5	.....	Drawn at 100° C. for 10 min.
F.....	82 100	51 500	46 000	23.7	30.8	112.0	.....	Drawn at 650° C. for 20 min.
Fig. 2 *A*.....	75 500	33 000	20 000	19.0	26.0	165.8	144.0	As cast.
B.....	97 150	59 500	54 000	10.9	17.8	110.2	247.0	Oil quenched.
C.....	91 750	54 500	51 000	21.3	40.5	107.0	195.0	Quenched and drawn at 500° C.
STEEL FORGING.								
Fig. 3 *A*.....	79 680	48 500	40 000	26.6	50.1	121.2	142.0	Annealed.
B.....	132 500	92 000	61 500	9.1	16.7	150.0	477.0	Special heat treatment.
RIVET STEEL.								
Fig. 4.....	50 000	35 500	34 500	40.5	72.6	102.9	887	.....
HOT-ROLLED STEEL.								
Fig. 5 *A*.....	70 000	34 000	28 000	26.8	40.1	121.4	.....	As rolled.
B.....	94 150	75 500	72 000	23.7	63.5	104.2	.....	Quenched from 800° C., drawn at 600° C.
STEEL DISK PLATE.								
Fig. 6.....	63 050	27 500	14 500	30.7	48.9	189.6	.....	.....
NICKEL STEEL.								
Fig. 7 *A*.....	75 750	55 000	53 000	32.3	56.0	103.8	139.5	Annealed.
B.....	85 530	73 000	69 000	27.1	58.2	105.8	162.7	Quenched from 850° C.; drawn at 650° C.

load applied to elongate it an amount equal to 0.635 per cent of its gage length (curve *A*), which was assumed to be in excess of the yield point. The load was then released and the specimen removed from the testing machine, aged by boiling in water for

10 minutes and again elongated 0.635 per cent (curve *B*). This procedure was repeated three times, as shown in curves *C*, *D* and *E*, after which the specimen was drawn at a temperature of 650° C. and the test terminated by breaking the specimen (curve *F*).

Fig. 1 presents two characteristic types of curves. The "gradual-curve" type, shown in curve *A*, has a long and gradual

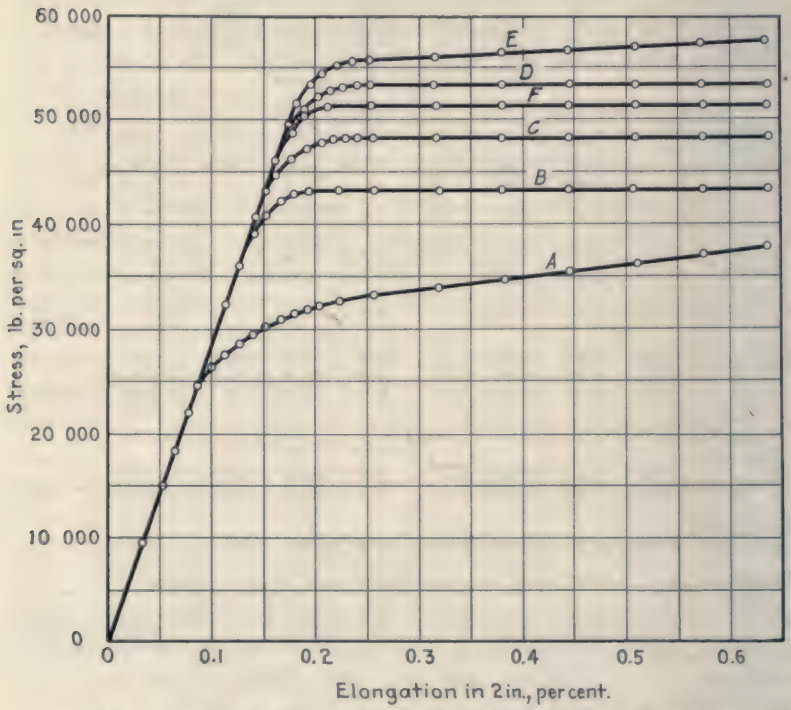

FIG. 1.—Stress-Strain Diagrams for Specimen Cut from Sand-Cast Coupon, without Subsequent Annealing or Heat Treatment.

curvature between the proportional limit and the yield point, while the "abrupt" type, shown in curves *B*, *C*, *D*, *E* and *F*, has a short and relatively sharp bend in the curve between the proportional limit and the yield point. The gradual curve shows a ratio of yield point to proportional limit of 144 per cent and the abrupt curves show ratios of approximately 112 per cent. Curve *A* represents that condition of the steel produced by cooling

slowly from the molten to a solid state, while curves *B*, *C*, *D*, *E* and *F* represent the condition produced by the application of mechanical work to the steel after having become cold.

Fig. 2 shows a set of curves plotted from the test records of three specimens cut from the same sand-cast coupon. Curve *A* represents the material as cast, without annealing or further

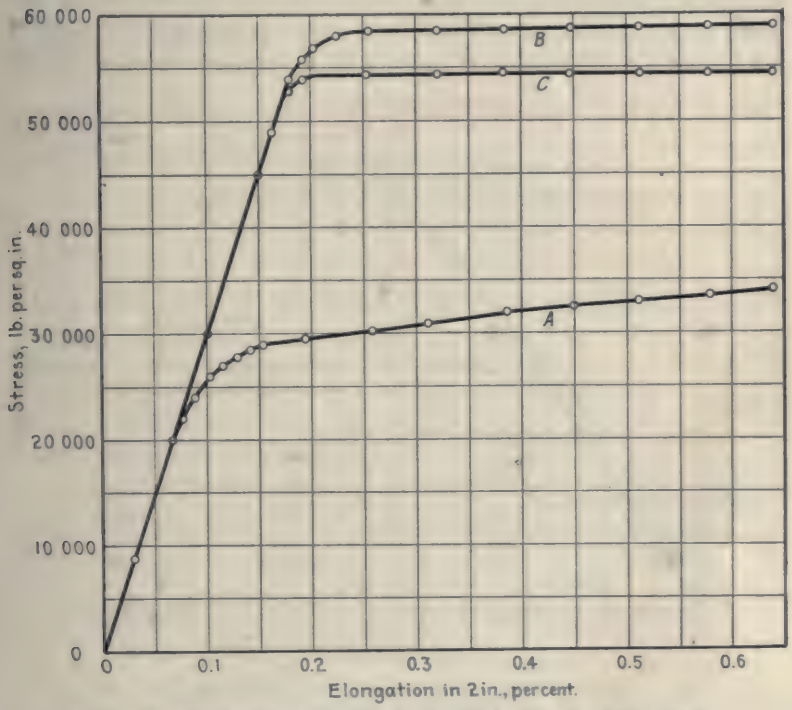

FIG. 2.—Stress-Strain Diagrams for Specimen Cut from Same Coupon as in Fig. 1.

treatment. Curve *B* represents the material after quenching in oil, and curve *C* represents the material after quenching in oil and then drawing at 500° C. The gradual curve *A* shows a ratio of yield point to proportional limit of 165.8 per cent and the abrupt curves *B* and *C* show ratios of 110.2 and 107 per cent, respectively. Here, again, the general curve *A* represents that condition of the steel produced by cooling slowly from the molten

to the solid state, while the abrupt curves *B* and *C* are produced, not by mechanical work, but by a simple heat treatment.

Fig. 3 shows a set of curves plotted from the test records of two specimens cut from a carbon-steel forging, having 0.475 per cent carbon. Curve *A* represents the steel as quenched and afterward carefully annealed and shows a ratio of yield point to proportional limit of 121.2 per cent, while curve *B* represents the steel in a condition produced by a very special heat treatment and shows a ratio of yield point to proportional limit of 150

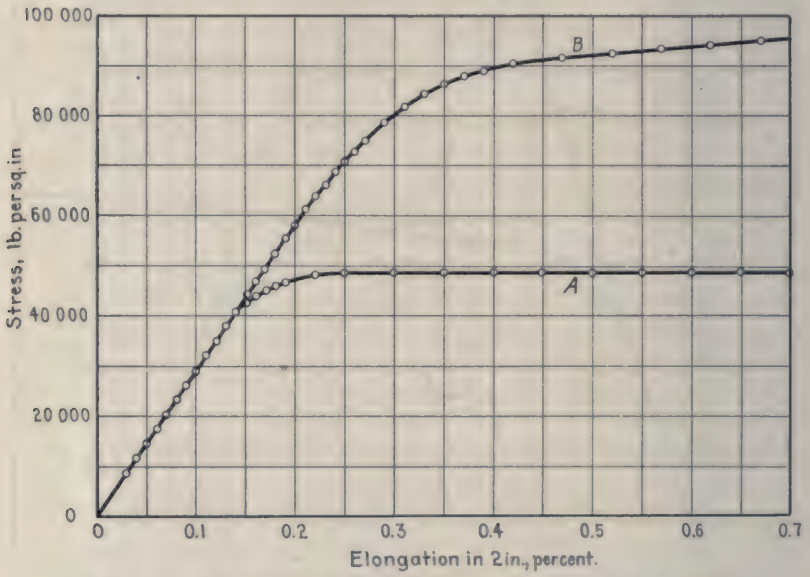

FIG. 3.—Stress-Strain Diagrams for Two Specimens Cut from a Carbon-Steel Forging.

per cent. The abrupt curve *A* has a very decided yield point while the gradual curve *B* does not give evidence of any decided yield point, but on the contrary shows that the specimen continued to resist the applied load throughout the test. To verify the peculiar resisting quality of the material, the testing machine was stopped several times with the accumulated load left on, when, contrary to our expectation, there was no further elongation of the specimen. Even after the sample had been elongated 0.5 per cent the stretching stopped as soon as the testing

machine stopped and began again the moment the machine was set in motion.

Fig. 3 is introduced to show that neither the drop-of-beam method, nor the use of ordinary dividers, can be relied upon to determine the yield point of steels having this special characteristic. In order to approximate the yield point of steels having this characteristic, the load per square inch necessary to elongate specimen 0.5 per cent has been arbitrarily adopted as the yield point for such steels.

Fig. 4 shows a curve plotted from the test records of a

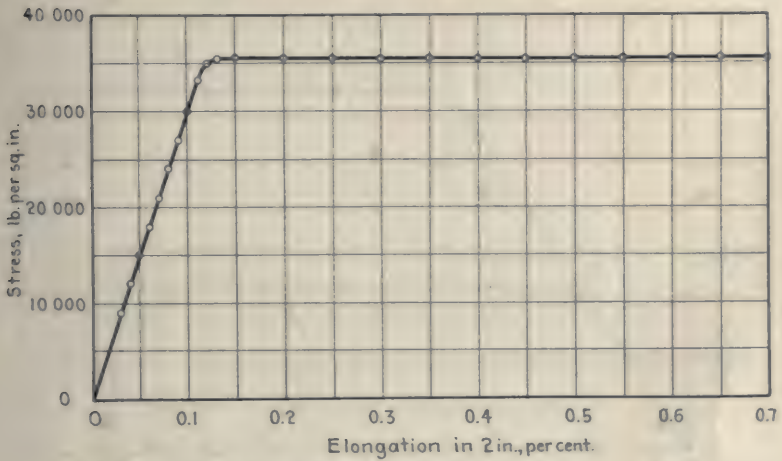

FIG. 4.—Stress-Strain Diagram for Specimen Cut from ¾-in. Rivet-Steel Rod.

specimen cut from a rod of rivet steel ¾ in. in diameter. This curve represents the material as received from the mill and has a ratio of yield point to proportional limit of 102.9 per cent. This abrupt type of curve is no doubt due to the fact that the rod had become relatively cold before receiving the final pass at the mill. Here cold working is again shown to produce the abrupt type of curve.

Fig. 5 shows a set of curves plotted from the test records of two specimens cut from a bar of axle steel 2¼ in. in diameter. Curve *A* represents the material as received from the rolling mill and has a ratio of yield point to proportional limit of 121.4

per cent. This ratio indicates that the material had cooled well below the critical range before receiving the final pass at the mill, but not finished at as low a temperature as the rivet rod referred to in Fig. 4. Curve *B* represents the material after having been properly heat treated and has a ratio of yield point to proportional limit of 104.2 per cent. It is interesting to note that the proportional limit shown by curve *B* is greater than the tensile strength shown by curve *A*, the difference being due entirely to heat treatment.

Fig. 6 shows a curve plotted from the test records of a

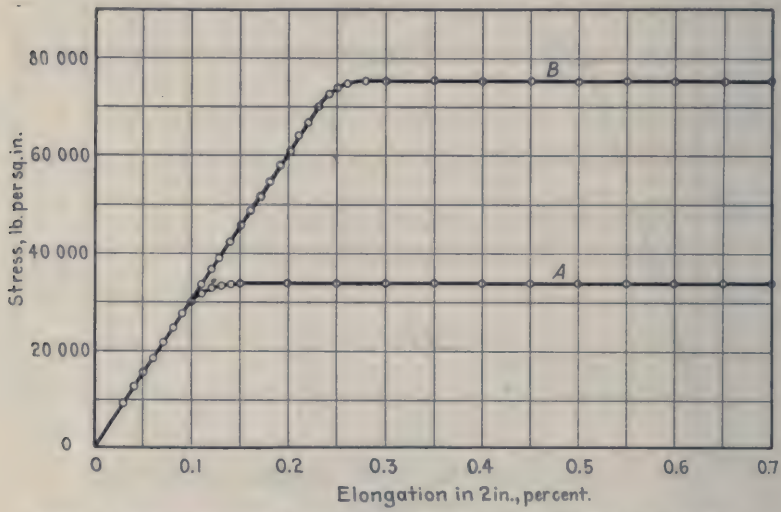

FIG. 5.—Stress-Strain Diagrams for Two Specimens Cut from 2¼-in. Steel Axle.

specimen cut from a large steel plate 2¼ in. thick. This curve represents the material as received from the rolling mill and has a ratio of yield point to proportional limit of 189.6 per cent. This high ratio verifies the fact that the plate was given its final pass at the mill while still very hot.

Fig. 7 shows a set of curves plotted from the test records of two specimens cut from a bar of nickel steel. Curve *A* represents the material in the annealed condition and has a ratio of yield point to proportional limit of 103.8 per cent, while curve *B* represents the material in the heat-treated condition and has

a ratio of yield point to proportional limit of 105.8 per cent both curves being of the abrupt type, which seems to be characteristic of all properly heat-treated alloy steels.

A review of the tests shown by the curves, as well as many test records not reproduced here, leads us to conclude that there

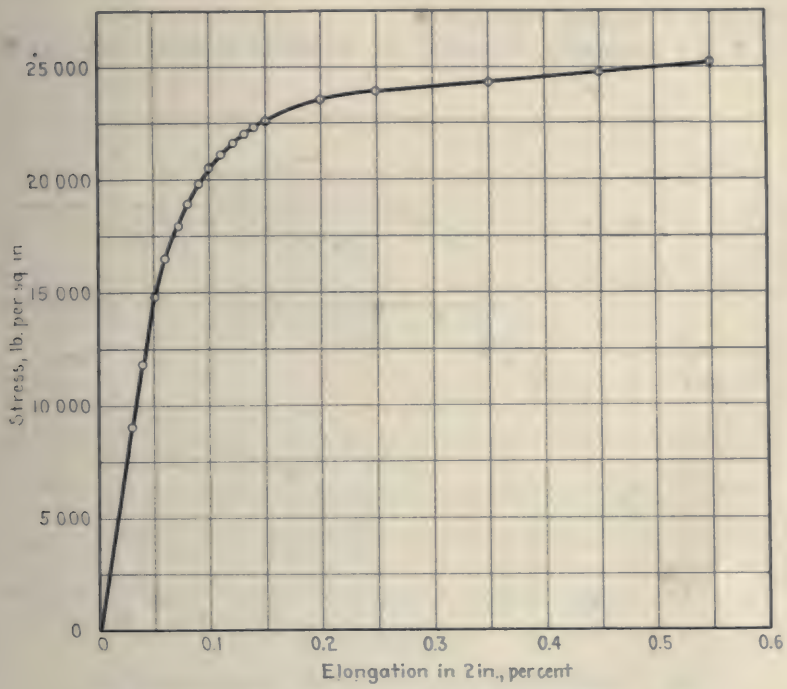

FIG. 6.—Stress-Strain Diagram for Specimen Cut from 2¼-in. Steel Plate.

are two main types of stress-strain curves produced from test records of steel, namely "gradual" and "abrupt."

The gradual curve presents a long and gradual curvature between the proportional limit and the yield point and is shown, in tests from untreated steel castings (Figs. 1 and 2), in tests from specially heat-treated steel forgings (Fig. 3), and in tests from heavy plates finished hot at the rolls (Fig. 6). Curves of this type may or may not indicate a decided yield point.

The abrupt curve presents a short and relatively sharp curvature between the proportional limit and yield point and is shown in tests from steel castings subjected to cold working or heat treatment (Figs. 1 and 2), in tests from rounds finished relatively cold at the mill (Figs. 4 and 5), and in tests from various grades of heat-treated steel. Curves of this type usually indicate a decided yield point.

It is important, therefore, for designing engineers who must

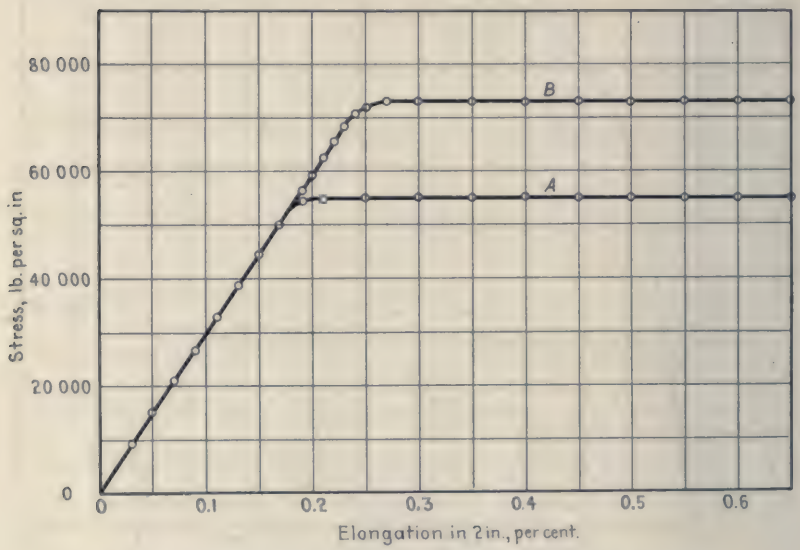

FIG. 7.—Stress-Strain Diagrams for Two Specimens Cut from Nickel-Steel Bar.

stress their material to a high degree, to know, at least approximately, as determined from a stress-strain diagram, the proportional limit of the material to be used. The advancement made in the heat treatments of steel during recent years has been so rapid and so important that engineers may well study the type of curve to be found in different grades of steel subjected to different heat treatments.

A study should include the correlation of the chemistry, manufacture, heat treatment and the resultant physical charac-

teristics shown in the actual curves. This knowledge of the material to be used, and equally accurate knowledge of the stresses to be met in service, should enable an engineer to so proportion his design and select the material and treatment, that the load applied will never be above the proportional limit of the material.

THE RELATION BETWEEN YIELD POINT AND PROPORTIONAL LIMIT IN VARIOUS GRADES OF STEEL.

PAPER NO. 3.

BY H. F. MOORE AND F. B. SEELY.

SUMMARY.

An examination of the data of carefully made extensometer tests shows that for actual materials Hooke's law is an extremely close approximation rather than a rigidly exact statement of fact.

All "limits of elasticity" depend for their determination upon sensitiveness of apparatus used, and are to some extent arbitrarily determined.

Various methods proposed as standard for locating one common limit of elasticity—the limit of proportionality of stress to deformation—are discussed, and test results are given involving the use of the various proposed methods.

THE RELATION BETWEEN YIELD POINT AND PROPORTIONAL LIMIT IN VARIOUS GRADES OF STEEL.

By H. F. Moore and F. B. Seely.

An examination of test data from various sources, actual experience with apparatus for measuring deformation, and a study of the literature of the subject have led the writers to believe that no absolute limit of elasticity has been determined for any actual material, and that for materials used in engineering practice Hooke's law is an approximation—an extremely close approximation for ductile materials, but still an approximation rather than a rigidly exact statement of fact. Any limit of elasticity, however determined, is regarded as a practical criterion for static elastic strength, rather than as the limit of absolutely elastic action for the material. For purposes of definition for testing engineers such a limit should be fixed at a stress for which some very small, but definite, deviation from elastic action is detected. No statically determined elastic limit fixes the "endurance" limit for a material, though in general, ability to withstand a great number of repetitions of stress accompanies high elastic strength.

When deformation measurements of extreme delicacy are made it is found that even with the most uniform of materials the effect of non-homogeneity tends to mask the effect of inelastic action. The effect of non-uniform stress distribution in the specimen also causes uncertainty. Apparent deviations from elastic action in such tests may be due to non-homogeneity of material, to initial stress, or to uneven stress distribution. The testing engineer cannot control the non-homogeneity of the material tested, nor can he eliminate entirely non-uniformity of stress distribution. Tests of extreme delicacy are of general interest, but it is not at all certain that the deviations from elastic action sometimes noted in such tests with small stresses are of any great significance in determining the working static strength of the material.

The stress at which a very small but definite permanent set can be detected is probably the most nearly logical "elastic limit," but the process of determining such a limit is very slow, and this limit has never been recommended as suitable for commercial testing. The yield point is at present the commonest limit used in practice. For rolled ductile metals, such as structural steel, it is a fairly satisfactory standard, though it is a matter of some difficulty to insure proper determinations of the yield point under practical laboratory conditions. For harder and less ductile rolled metals and for cast metals, the location of the yield point is very indefinite. The proportional limit (of stress to deformation) promises to be the most generally useful of all the proposed limits of elastic strength, if some method of determination of the proportional limit can be recognized as standard—a method which shall yield consistent results, and which shall not be too complicated nor too slow for use in commercial testing laboratories.

Various methods of determining the proportional limit have been proposed as standard. The French Commission on Methods of Testing the Materials of Construction, whose report was published in 1895, recommended that in determining this limit deformation measurements be made to the nearest 0.001 mm. (0.00004 in.). The late J. B. Johnson[1] proposed that an "apparent elastic limit" be used as a standard and that it be located at that stress for which the rate of deformation (slope of stress-strain diagram) is found to be 50 per cent in excess of its initial value (value for very small stress). In the recommendations of Committee E-1 on Methods of Testing of this Society presented at the last annual meeting,[2] it was proposed that in determining the proportional limit deformation readings should be taken to the nearest 0.0001 in. A method which has been found to give fairly consistent results in the Materials Testing Laboratory of the University of Illinois consists in locating the proportional limit at that stress for which the stress-strain diagram shows a deviation from a straight line amounting to 0.0001 in. per inch of gage length. It seems to the writers that the standard methods proposed by the French Commission and those presented to this Society in 1915

[1] "The Materials of Construction," p. 18.

[2] *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part I, p. 443 (1915).

by Committee E-1 are open to criticism in that both methods give, for differing gage lengths, differing limiting values for the unit deformations. Johnson's method and the method used in the laboratory of the University of Illinois give results for proportional limit independent of the gage length.

It should be noted that specifying the precision of instru-

TABLE I.—DETERMINATION OF PROPORTIONAL LIMIT BY VARIOUS METHODS.

Material.	Gripping Device.	Number of Test Specimens.	Proportional Limit, lb. per sq. in.				Yield Point, lb. per sq. in.[3]
			Deformation Readings to 0.00001 in. per Inch of Gage Length.[1]	Deformation Readings to 0.0001 in.; Total Elongation.[2]	Deviation from Straight-Line Law of 0.0001 in. per Inch of Gage Length.	Johnson's "Apparent Elastic Limit."	
Soft-steel rod...	Threaded-end test specimens with spherically seated holders.....	2	32 200	36 200	35 700	35 000	40 000
Soft-steel rod...	Wedge grips; good alignment.....	5	30 700	35 500	34 400	34 000	38 000
Soft-steel rod...	Wedge grips, upper pair 0.035 in. out of line.....	2	31 000	35 000	35 400	34 700	37 200
Soft-stee rod...	Wedge grips, upper pair 0.070 in. out of line.....	2	31 000	33 300	33 900	33 200	37 000
Tool-steel rod..	Threaded-end test specimens with spherically seated holders.....	4	55 000	61 500	62 400	61 400	66 400
Tool-steel rod..	Wedge grips: good alignment.....	2	57 500	59 300	60 700	59 700	65 400
Tool-steel rod..	Wedge grips, upper pair 0.035 in. out of line.....	2	50 700	58 000	57 300	56 500	63 000

[1] The French Commission on Methods of Testing the Materials of Construction recommended readings to 0.001 mm. and for specimens about ¼ in. in diameter used a gage length of 100 mm. Their method then corresponded to the method used—readings to 0.00001 in. of gage length.

[2] This corresponds to the method suggested by Committee E-1 on Methods of Testing. As the gage length was 2 in. for all specimens, a total elongation of 0.0001 in. corresponds to a unit elongation of 0.00005 of the gage length.

[3] The yield point was located by the sudden "jump" of the dial of the extensometer, rather than by the drop of the beam of the testing machine.

ments to be used, arbitrarily determines what amount of inelastic action shall be considered negligible, and that all the above methods are arbitrary. Any one of them would determine as the proportional limit a stress under which the material showed small, but definite, inelastic action.

Recently the writers have begun a series of tests to determine

by various methods the variation in the value determined for the proportional limit, and also the variation in value determined which is caused by unevenness of gripping of test specimens. Data have been secured for specimens of soft-steel rod, and for specimens of tool-steel rod. These data are given in Table I. Figs. 1, 2 and 3 show some typical stress-strain diagrams obtained. All the specimens of one kind of steel were cut

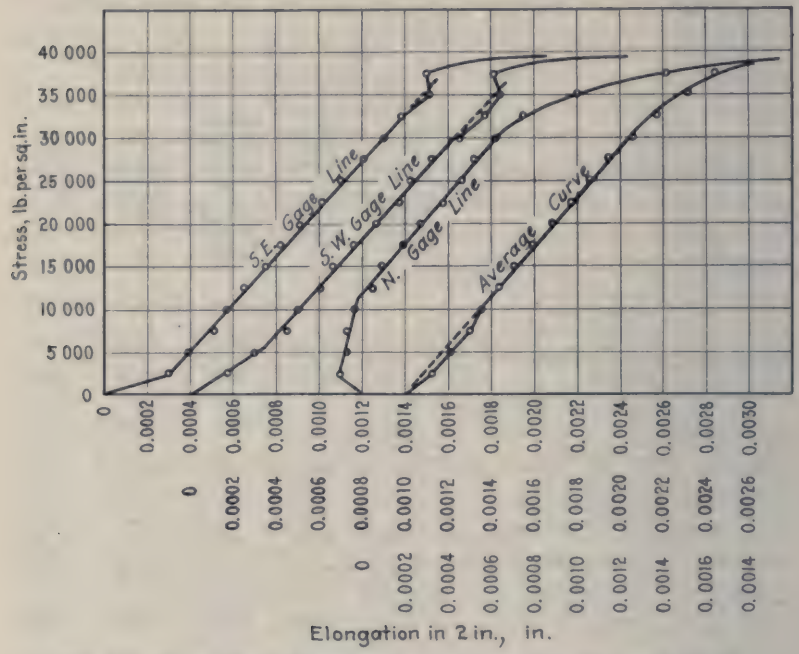

FIG. 1.—Stress-Strain Diagrams for Specimen with Threaded Ends and Spherical-Seated Holders.

from the same rod. The specimens were slightly longer than the standard 2-in. specimen to allow room for using a Berry strain gage on the specimens held in wedge grips. Variation in evenness of gripping was secured by using threaded-end specimens and spherical-seated holders for some specimens, wedge grips for other specimens, and for still other specimens, wedge grips with one pair of wedges thrown out of line by means of shims. In some tests, elongations were measured by means of

a 2-in. "averaging" extensometer, in which the average stretch was indicated on one micrometer dial gage; for other specimens, elongations were measured along three longitudinal gage lines, symmetrically placed, by means of a 2-in. Berry strain gage. While the data are too few to be used as the basis for quantitative conclusions they give some fairly definite qualitative indications.

From the comparison of strain-gage readings along indi-

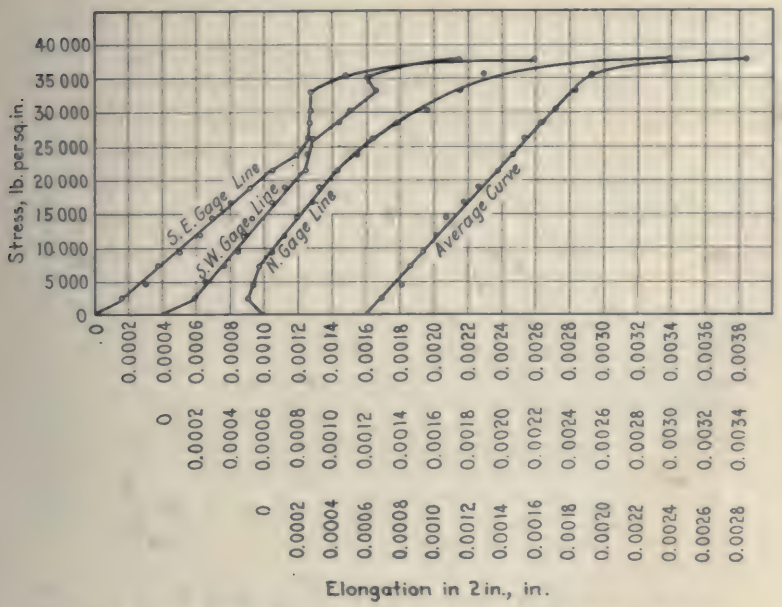

FIG. 2.—Stress-Strain Diagrams for Specimen Held in Wedge Grips with One Pair of Wedges 0.035 in. out of Line.

vidual gage lines with the average deformation in a test specimen, it seems that there may be considerable variation in the action along individual gage lines, without causing the average stress-deformation diagram to deviate appreciably from a straight line.

The readings of an "averaging" extensometer give practically the same stress-strain diagram as the diagram plotted from the average of the readings along three gage lines, symmetrically placed.

Evenness of stress distribution is probably never completely attained even with threaded-end specimens and spherical-seated holders. With such a gripping device very carefully adjusted, appreciable variation was observed in the stresses along the three gage lines.

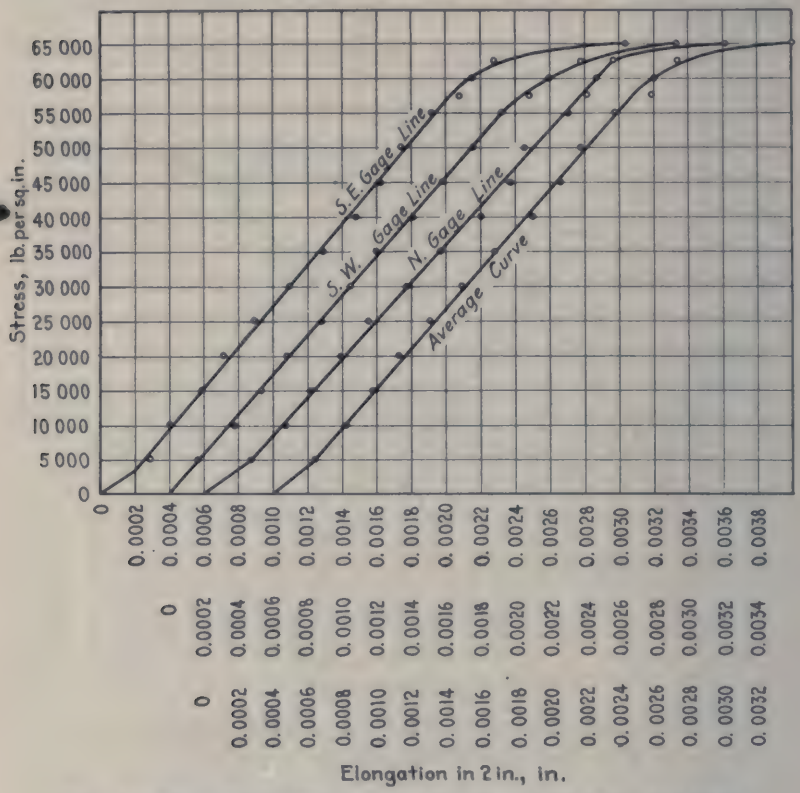

FIG. 3.—Stress-Strain Diagrams for Specimen with Threaded Ends and Spherical-Seated Holders.

Lack of alignment of upper and lower gripping devices is only one of the possible causes of unevenness of stress distribution. Another important cause of this unevenness lies in the transmission of stress from the gripping device to the specimen, unevenness in bearing of threads in a threaded-end specimen, unevenness of hold of wedge grips, etc.

Values of the proportional limit determined from a single gage line may be far from representative for the material.

Comparing variation of individual tests from the averages given in Table I, Johnson's method, or the use of a deviation from the straight-line law amounting to 0.0001 in. per inch of gage length, gave slightly more consistent results for proportional limit than did the two other methods used.

The values of the proportional limit determined from average elongation for test specimens with threaded ends and spherical-seated holders do not differ greatly from the values found with wedge grips in good alignment. This would indicate that considerable variation in evenness of gripping may take place without greatly affecting the value of the proportional limit determined.

The value of the proportional limit is dependent on the sensitiveness of measuring apparatus used. It is also dependent on the scale used in plotting results of tests. In the tests reported a fairly large scale was used.

The writers hope to carry these tests further, and suggest that a study of the evenness of stress distribution for different types of gripping devices, and of the effect of unevenness of stress-distribution on the value of the proportional limit, is an experimental problem involving comparatively little expense for apparatus or specimens, and one whose solution is of prime importance.

GENERAL DISCUSSION.

Mr. Morse.

MR. H. L. MORSE (*presented in written form and read by the Secretary*).—The discussion of the relation between yield point and proportional limit, which has so far been brought out, is far from settling what appears to me to be a serious confusion of ideas among engineers and manufacturers existing at the present time.

The situation to be met is that for certain classes of machines or apparatus, a very low factor of safety must be used in order that for the power required the weight of the resultant structure may not be prohibitively great. The designer works on a certain pre-determined elastic limit, which, as Mr. Howard has very justly stated, must be a definite and exact quantity on which absolute reliance may be placed. Then the metal is accepted on tests purporting to give the elastic limit, and it is found that stresses of 70 per cent of this amount or less cause permanent sets in the material, due to the fact that the *yield point* and not the elastic limit has been the criterion in accepting the material. It appears to me that results of this nature, which are by no means uncommon at present, require very definite and certainly prompt efforts in the direction of their elimination.

I believe that the principal trouble lies in the terminology, and that it is essential in the first place that a term such as "elastic limit," which has come to mean in common parlance anything between the "*true* elastic limit"—which is probably what we also term the "proportional limit"—and that very vague quantity known as the "yield point" or "drop of the beam," that this term, although having, as Mr. Howard has said, an inherently definite meaning, should be abandoned on account of the inaccuracy of meaning which has been allowed to develop in its use.

It is beyond question that certain metals, generally those of comparatively small mass, may be heat-treated in such a way as to bring the proportional limit and yield point to

practical coincidence; but it is equally true that poorly heat-treated metals of the same mass and metals of greater mass with the best possible heat treatment will give a very distinct proportional limit, sometimes as low as 50 per cent of the figure properly taken for yield point, and varying from this figure to the practical coincidence noted above. Mr. Morse.

As long as the term "elastic limit" is in common use, we shall find many designers, manufacturers and careless investigators using that term to denote the yield point, because the second point of the graph has been carelessly termed the "*commercial* elastic limit," and by dropping the qualifying adjective "commercial" the two very distinct points of the graph have come to be designated by the same term.

An engineer of tests at Watertown Arsenal, in one or two instances which I have happened to run across, selected as the elastic limit a stress which was really the yield point, and I believe without knowing that he did so, for the reason that he drew no graph of the results of his test, and picked the point of departure from proportionality by inspection of the differences between successive elongations and did not note the very slight increase in these differences which occurs at the proportional limit.[1]

The definitions of "elastic limit" and "proportional limit" recommended by Committee E-1 on Methods of Testing for adoption by the Society neither state nor deny the inherent identity of significance of the two terms.[2] If the proportional limit is taken as contemplated in these definitions, and is determined from a graph of the test results, it would appear to be nearly independent of the degree of precision of the extensometer used, for the reason that a graph correctly representing a sufficiently large number of points obtained with an extensometer of low precision will be identical with the graph made by using an extensometer of high precision, although in the first case none of the points may lie on the graph and in the

[1] Tests of Metals, 1894, pp. 42 and 68; Tests of Metals, 1906, p. 347.

[2] The definitions here referred to are given for convenience:

Elastic Limit is the greatest load per unit of original cross-section which does not produce a permanent set.

Proportional Limit is the load per unit of original cross-section at which the deformations cease to be directly proportional to the loads.—ED.

Mr. Morse. second all may lie practically thereon. The elastic limit, however, as defined by Committee E-1, is absolutely dependent on the degree of precision of the extensometer, and it will be the higher the less sensitive the extensometer.

The "yield point" and "drop of the beam" should be identical, though much vagueness exists of necessity in either of these characteristics, owing to the fact that with any but the softest metals the yield point is not distinct, and the "drop of the beam" under even a slow speed of testing is with difficulty, if at all, distinguishable. It would seem that for commercial use an extensometer of some sort should be used to determine the yield point and its exact location then defined by a certain standard amount of departure from the Hooke's-law line.

This brings me to a question not mentioned in any other discussion so far brought to my attention. Mr. Lynch's most valuable curves do not meet my requirements, as his points lie on the curve with to me most disconcerting regularity, and the curves with similar regularity pass through zero. Many of the graphs obtained in recent tests of steel at Watertown Arsenal consist of two straight lines and a curve. The Hooke's-law line does not pass through zero, but generally through a point slightly to the left. At the proportional limit a new straight line begins, which extends to the yield point at an angle of 5 to 10 deg. with the Hooke's-law line. At or just below the yield point this second straight line breaks into a curve which may be very abrupt or very gentle. Of these three branches the first represents essentially elastic deformation. The second appears to represent purely elastic deformation plus a plastic deformation proportional to load increments, and the curve represents the breaking down of the specimen with plastic deformation predominating.

Several variations from this general case are found to recur at frequent intervals, of which I may mention one in which the second straight line is nearly parallel to the Hooke's-law line and offset from it through a short curved branch. Another is the case of poorly heat-treated or cold-worked metal in which there is no appreciable length of Hooke's-law line, the graph being a curve practically from zero. The case previously

mentioned, where the proportional limit and yield point are practically coincident, is a special case which we are fortunately able to obtain by suitable heat treatment of reasonably small masses. Mr. Morse.

I have not touched on the exact topic of the discussion for the reason that it seems to me necessary first to establish a fundamental basis of exact terminology and accepted practice. The definitions of the tensile characteristics under discussion, as proposed by Committee E-1, note that the proportional limit, as well as the elastic limit, are rarely determined "in the commercial testing of materials."

If the yield point is to remain the characteristic ordinarily determined in commercial testing, then it is believed that it should be more exactly defined, and an extensometer utilized in its determination. As the elastic limit or proportional limit is the essential characteristic to the designer, it must be known, at least approximately, by the inspector on whom the duty of accepting or rejecting materials is imposed.

There are, therefore, two alternatives. The first is the determination of the elastic or proportional limit by reasonably accurate testing methods as the essential criterion of acceptability of a large class of metals for important purposes and important machines in the design of which a low or moderate factor of safety is employed; the second is the determination for any given class of metal, considering both chemical composition and heat treatment, of the relation between the yield point and the proportional limit, and it would seem that this would also require an examination of each particular type of testing machine and speed of testing; then by determination of the yield point the probable proportional limit of the material under question could be deduced.

It seems to me that the first of the two alternatives is by far the more preferable, especially as the determination of proportional limit is not by any means a tedious or difficult operation with a suitable extensometer. This, of course, requires drawing a graph for each specimen tested.

The following suggestions are therefore submitted, for reasons touched on herein:

1. That the term "elastic limit" be abandoned as far

Mr. Morse. as possible, having lost its definite meaning through abuse, and that the term "proportional limit" be substituted therefor.

2. That the "proportional limit" be required as a portion of the acceptance test for all materials which are to be incorporated in parts designed with low or moderate factors of safety.

3. That the "proportional limit" be obtained by plotting a suitable number of points and drawing a graph representing those points, the proportional limit being determined as that point where the Hooke's-law line intersects a second straight line drawn to the curve which represents the yield point of the metal.

Mr. Devries. MR. R. P. DEVRIES (*presented in written form and read by the author*).—This discussion aims to treat briefly of a few of the points taken up in the three papers presented. The points enumerated will follow the printed order of the three papers.

Referring to Mr. Howard's paper, under the heading "Speed of Testing," the statement occurs that "The strains which are displayed by the steel correspond to the stresses, however rapid the application or alternation of loads may be." If this is correct it is of the utmost importance. Reynolds and Smith,[1] making alternating bend tests, as well as Bairstow,[2] making alternating tension and compression tests, concluded that the resistance of the metals varies in inverse proportion to the rapidity of the reversals of stresses. Hopkinson[3] and Roos[4] arrived at opposite conclusions. Stanton,[5] and Eden, Rose and Cunningham[6] conclude there is no relation between speed and endurance if the number of alternations per minute does not exceed 2000. As far as I have reviewed Mr. Howard's published data his speeds never reached even 2000 r.p.m. Alexanderson,[7] in describing a 100,000-cycle generator in which the speed of the rotor mounted on a shaft must attain 20,000 r.p.m., states that when he had an unbalanced pull of 80 lb. on his rotor sufficient to bend the shaft at standstill the machine

[1] *Philosophical Transactions*, Vol. 199, p. 265 (1902).
[2] *Philosophical Transactions*, Vol. 210, p. 35 (1910).
[3] *Proceedings*, Royal Society, Vol. LXXXVI, p. 142.
[4] *Proceedings*, Inter. Assoc. Test. Mats., 1912 Congress, Vol. 2, Part II, Section A.
[5] *Transactions*, Inst. Civ. Engrs., Vol. 66, p. 78 (1906).
[6] *Proceedings*, Inst. Mech. Engrs., 1911, Parts 3–4, p. 871.

continued to run as if nothing had happened. The rotor had a clearance of only 0.015 in. If the speed of application in static testing has no effect on the strains, elastic hysteresis cannot of course exist. Mr. Devrie

Yield Point.—The yield point as defined by Committee E-1 of this Society should evidently not be defined if, as stated by Mr. Howard, it "is essentially vague and uncertain;" and it would undoubtedly be better not to define it at all if the following statement of his is correct: "The most that can be said about it is that a structural member loaded up to its yield point will suffer a certain amount of permanent strain."

However, when the yield point as defined by Committee E-1 is considered, it becomes evident that some engineers do not view this matter in the same way. If certain structural steel, for instance, would show no yield point, it would probably be a matter of interest to the structural engineer and it would not be a wholly theoretical interest either. Steel and many other metals may be considered as consisting of certain particles which are held in a state of equilibrium through mutual reactions. For a certain interval of external stress application originating at zero stress, these particles yield according to an apparently well-defined law which is usually expressed by saying that stress and strain are proportional. When they no longer follow this law the proportional or elastic limit is exceeded. For all stresses above the elastic limit the steel extends according to another law which is undoubtedly more complex than the first. In this region of flow under applied stresses important changes are taking place which can be recognized as distinct slipping of the crystal grains. For steels possessing a yield point it is altogether possible that this point marks a distinct ending of some state of flow.

The yield point is probably no more of an artificial point in the stress-strain diagram than the critical points obtained when certain steels are slowly cooled from high temperatures. Moreover, the metallurgical engineer does not discard the information obtained from a study of the critical ranges found in certain steels because certain other metals have no critical ranges. It is in this connection that Mr. Lynch's paper is extremely timely, for he calls attention to a much neglected

Mr. Devries. portion of the stress-strain diagram, namely, that portion lying beyond the elastic. He defines the different states of the steel under test by using a ratio of stress at a certain combined elastic and permanent strain to the stress at the proportional limit. While appreciating fully the difficulties of using in an intelligent manner the information that may be gained from a study of the so-called plastic-flow portion of the stress-strain curve, it should be made clear that the method used is arbitrary. The curves of Fig. 3 in his paper illustrate why taking 0.5 per cent as a criterion of plastic flow is not a good index. In curve *A*, approximately 0.14 per cent of the strain is elastic and the remaining 0.36 of the 0.5 per cent is probably partly elastic and partly plastic. In curve *B*, 0.21 per cent is wholly elastic and 0.29 per cent is partly elastic and partly plastic.

Messrs. Moore and Seely have seen fit to confine themselves largely to the conditions and methods of testing. In connection with their Fig. 2, it would have been interesting to see whether the conditions there depicted do not lower the proportional limit by local overstressing, even when the average curve is a straight line.

On the whole it is extremely unfortunate that the correct determination of modulus of elasticity is not so easy a matter as is sometimes assumed. If moduli of elasticity could always be certified as being accurate to within one per cent the study of overstressed material would receive a new impetus.

Mr. Moore. MR. H. F. MOORE.—In connection with the question of identity of the elastic limit (stress for first permanent set) and the proportional limit, the speaker would like to state that for miscellaneous material as received by the University of Illinois Materials Testing Laboratory it is the general experience that the elastic limit as determined by tests is not the same as the proportional limit as determined by tests.

Mr. Fry. MR. L. H. FRY.—I think that the papers and the discussions have brought out the necessity for considering the relation between the elastic limit and the proportional limit. Mr. Howard says that the elastic limit and the proportional limit are synonymous, and I think, that on this point at least, Mr. Devries agrees with him. If they are not the same we must be able to get some idea of the difference between the physical conditions at the two

points. I have searched diligently and have been unable to find any definition which would show any difference. In Howe's "Metallography of Steel," it is stated that "the passing of the elastic limit means that the stress reaches such intensity that certain crystallin slices, forming part of certain crystallin grains, start to slide over the similar neighboring slices in those same grains." It seems to me that this is an equally good definition of the condition at the proportional limit as at the elastic limit. I have noticed in one of Mr. Moore's papers presented to the Society, that as the refinement of his methods of measuring increases, there appears to be an increase in the difference between the proportional limit and the elastic limit, but I should like to ask whether this is not due to the fact that, in measuring the proportional limit, the lengths to be measured are considerably larger than the lengths to be measured in determining the elastic limit, where we are measuring only the permanent set. The evidence seems to be that the two values are really the same. If this is true we should have a very good scientific reason for the method adopted by Committee A-1 for determining the elastic limit in commercial work. This method is practically a determination of the proportional limit, although in deference to well-established practical usage the property determined is spoken of as the elastic limit. If the elastic limit and the proportional limit are fundamentally the same, the method adopted by Committee A-1 for determining the elastic limit for the purposes of specifications, besides being satisfactory from a practical point of view, would be scientifically correct. **Mr. Fry.**

MR. MOORE.—In discussions of elastic limit the speaker has generally used the term elastic (or proportional) limit *as determined by the test*, referring to the limit which was actually determined in a test for a particular sample of material as received. There has usually been found a difference between the two limits (elastic limit and proportional limit) for a material; this difference may be due to initial stresses in the material, to limitations of precision of apparatus, or to other causes. **Mr. Moore.**

MR. C. D. YOUNG.—This has been a most interesting discussion, especially in view of the trials and tribulations of Committee A-1 during the past year. We started with the term "elastic limit," and so far, we have retained it without change, **Mr. Young.**

Mr. Young. but we have now reached the point where it is necessary to define it more clearly. There are two purposes for which elastic limit or proportional limit or yield point is of use; first, to the designer in deciding what material he will select for the particular construction he has in mind; and second, to the testing engineer who must test the material after the designer has made his selection. Now, in the first case, it is a relatively easy matter for the designer to test a number of materials, plot diagrams, find the proportional limits and call them proportional limits or, if you please, elastic limits. Having established these elastic limits, he can select a material which seems to satisfy the conditions for the design. He may then specify, we will say, a large tonnage of this material from eight or ten manufacturers, and ask a testing laboratory to test the material under specifications which he has prepared. It then becomes the problem of the engineer of tests to sample and test the material and to find in a few minutes and for a large tonnage, what the designing engineer found in weeks on a few pieces. I think that the members will readily appreciate the difference in the two problems.

When the testing engineer has four or five mills with large shipments of material waiting to be loaded and is told: "You are blocking the mill and we must have test results," he must find rapid tests which will produce approximately what the original tests did, and let the manufacturer release the material.

It is the purpose of Committee A-1 to adopt a test of which they can say: "This is a test which will give you very closely, if not exactly, the same thing as the original designer found by weeks of study in plotting points, releasing the load and going back and tediously determining the elastic limit." I think you should bear with these terms and endeavor to appreciate the commercial side of the problem as well as its engineering side. The author of the last formal discussion seemed to disregard entirely the viewpoint of the man who must test the material as compared with the man who is selecting the material for design, in so far as the speed of determining whether the material offered meets the specifications is concerned.

The Chairman. THE CHAIRMAN (PRESIDENT MANSFIELD MERRIMAN).—If there are no other remarks, the Chair would say a word regarding the importance of precise and consistent language. We

have "elastic limit," we have "proportional limit," and we naturally would expect to have "yield limit." Instead of that, we have "yield point." This is one of the anomalies of the English language; it does not occur in the German or French, and I think one of the functions of this Society in future years would be to get rid of that word "point." We have elastic limit; we have proportional limit; we should have yield limit. We might regard all three of these as "elastic limit" in a certain sense, because it is in that neighborhood, that the elastic properties of the material are disturbed. We might say that the first one is the "set elastic limit;" it is the stress where the permanent set first begins to be observed. The second one we might call "proportional elastic limit;" it is that stress where the strain ceases to be proportional to the stress. Then the third one, we might call the "yield elastic limit;" it is the stress where the strain continues to increase for a short time without increase in stress. These are mere suggestions for more precise and consistent use of language, which, as the years roll on, may perhaps be taken up by some committee. **The Chairman.**

MR. G. R. OLSHAUSEN (*by letter*).—In connection with the definitions recently recommended by Committee E-1 for elastic and proportional limits, the question arises whether there is any difference in the magnitude of these limits; and if such is the case, under what conditions this occurs. To decide this question it is convenient to suppose the total deformation L of a homogeneous test specimen, in tension or compression, to be made up of two components, the elastic deformation l_e and the permanent deformation, or set, l_s, so that **Mr. Olshausen.**

$$L = l_e + l_s.$$

The deformations l_e and l_s are functions of the applied load per unit of area, or stress, S, and the existence of either or both limits depends on the manner in which these deformations vary with the stress S. From the definition of the elastic limit it is evident that for its existence we must have

$$l_e = 0 \text{ when } 0 < S \leqq E.$$

Mr. Olshausen. and similarly, from the definition of proportional limit, we must have

$$L = kS \text{ when } 0 < S \leqq P,$$

where E and P are the applied stresses at the elastic and proportional limits respectively, and k is a constant.

TABLE I.—CONDITIONS FOR EXISTENCE OF PROPORTIONAL AND ELASTIC LIMITS.

Limits Existing.	General Conditions Necessary.	Particular Cases Possible.
Both *E*-Limit and *P*-Limit	$l_s=0$ when $E>S>0$; $l_s\neq 0$ " $S>E$	No. 1. $P>E$: $l_e=kS$ when $S\leqq E$; $l_e\neq kS$ " $S>E$
	$L=kS$ " $P\geqq S>0$; $L\neq kS$ " $S>P$	No. 2. $P=E$: $l_e=kS$ " $S\leqq P'\geqq P$; $l_e\neq kS$ " $S>P'$
		No. 3. $P<E$: $l_e=kS$ " $S\leqq P$; $l_e\neq kS$ " $S>P$
P-Limit	$L=kS$ when $P\geqq S>0$; $L\neq kS$ " $S>P$	No. 4: $l_e=0$ when $S\leqq P'\geqq P$; $l_e\neq 0$ " $S>P'$
No *E*-Limit	$l_s>0$ " $S>0$	No. 5: $l_s=aS$ " $S\leqq P<P'$; $l_e=bS$ " $S\leqq P'$; $a+b=k$
		No. 6: $l_s=aS$ " $S\leqq P'$; $l_e=bS$ " $S\leqq P<P'$; $a+b=k$
		No. 7: $l_s\neq aS$, $l_e\neq bS$, $S\gtrless P$
No. 8 — No *P*-Limit	$L\neq kS$ when $S>0$	
No. 8 — *E*-Limit	$l_s=0$ " $E>S>0$; $l_s\neq 0$ " $S>E$	
No. 9 — No *P*-Limit	$L\neq kS$ when $S>0$	
No. 9 — No *E*-Limit	$l_s>0$ " $S>0$	

Table I shows the conditions which must be satisfied in order that both, only one, or neither may exist. In this table P' designates a stress, equal to or greater than P, where either l_e or l_s change from zero or from direct proportionality with S to any other function of S, and a and b are constants. The symbol $\neq$ is used to indicate inequality.

Most of the nine cases enumerated in the table require that l_s and l_e vary according to laws which are not met with in any

known materials. This will become evident from the following discussion of the several cases. Mr. Olshausen.

Cases Nos. 1 and 7.—It is very improbable that case No. 1, for which the proportional limit is beyond the elastic limit, occurs at all, since the condition must be satisfied that $L = kS$ between the two limits; that is to say, after the material has taken a set the elastic and permanent deformations must vary in this interval in a manner such that their sum is directly proportional to S. For the same reason case No. 7 probably never occurs.

Cases Nos. 3 and 8.—In these cases the elastic deformation ceases to be proportional to the stress before the elastic limit of the material has been reached, that is, before there is any set. This is contrary to our experience.

Case No. 4.—This case supposes the material to be plastic and to have no elastic properties whatever in a certain stress-interval beginning with zero stress. This case is of little interest for the purposes of this discussion.

Case No. 9.—The material is supposed to have an elastic deformation and a permanent deformation, neither one, nor even their sum, being proportional to the stress. Soft materials such as lead probably represent this case.

Case No. 2.—This is the most important case. It occurs in materials for which the elastic deformation is proportional to the stress up to or beyond the elastic limit. It is most probable that this is the only case where both limits exist, the limits being identical.

Cases Nos. 5 and 6.—These cases are represented by a class of materials for which both elastic and permanent deformations are directly proportional to the stress. Some recent experiments seem to indicate that examples of materials having approximately these properties occur in practice.

This discussion establishes the fact that the most probable case where both elastic and proportional limits occur is the one in which they are identical; both terms, "elastic limit" and "proportional limit," should be retained, however, in order to indicate in what manner the determination of the limit was made.

MR. J. A. CAPP (*by letter*).—In the *Quarterly Mathematical Journal* of April, 1855, Sir William Thompson (Lord Kelvin) discussed at some length the thermo-elastic properties of mate- Mr. Capp.

Mr. Capp. rial, and showed mathematically that elastic matter when stressed absorbs heat, that is, its temperature as indicated by a thermometer is lowered; but, when the stressing is carried beyond the elastic limit and strain becomes permanent, the body gives out heat, that is, its temperature as indicated by a thermometer rises.

Mr. C. A. P. Turner presented a paper before the American Society of Civil Engineers[1] giving the results of a series of tests using the thermo-electric method of determining stress-strain relation, and a brief paper was also given by Mr. E. Rasch in 1909 in the Proceedings of the International Association for Testing Materials at the Fifth Congress. Professor Lawson and the writer gave results of a number of tests by the same method before the International Association for Testing Materials, Sixth Congress, in 1912, but the results were not entirely conclusive because we had not at that time succeeded in making the temperature indicating devices sufficiently sensitive. At that time we were endeavoring to use thermo-couples to indicate the temperature of the test piece, but we afterward found that by using the resistance measurement of temperature we were able to get a much more sensitive indication of the temperature changes in the test piece. We used substantially pure nickel wire, silk insulated, supplied for the purpose by Leeds & Northrup, and found that we could either make a small spiral coil and clamp it in contact with the test piece, or we could wind the wire around the test piece, and by proper heat insulation to prevent undue influence of external temperatures we were able to indicate the temperature changes in the test piece very sensitively. Tests made by this method on test pieces to which a Ewing Extensometer was attached, gave results which showed that the reversal of temperature of the test piece from heat absorption to heat emission occurred sharply at the limit of proportionality as determined by the Ewing Extensometer. Pressure of other matters forced us to lay aside further experiments with this very interesting method of test, and mention is made of it now in the hope that some of those who from the nature of their remarks have been giving very serious consideration to the subject of the elastic properties of materials, will find

[1] *Transactions*, Am. Soc. Civ. Engrs., Vol. XLVIII (1902).

opportunity to take it up, as it is the writer's opinion that the temperature method of measurement may throw considerable light on this very interesting subject. The relatively small amount of work which we have been able to do, however, seems strongly to indicate that the limit of elasticity and limit of proportionality have the same value, and that neither can be confused with the yield point if sufficiently accurate means of determination are used. The heat evolution at the yield point is enormous relative to the rate of heat emission from and at the limit of elasticity. Mr. Capp.

After we were forced to drop our work, we were advised of excellent papers on this subject by Professor E. G. Coker of the Technical College, Finsbury, and we are able to cite reference to some of his papers.[1]

It is earnestly hoped that the workers of the Bureau of Standards particularly will find opportunity to pursue this method further, as it is believed that the method may be worked out to a practical basis so that it may become applicable as a commercial method of testing to determine the elastic limit without having recourse to the much more cumbersome extensometer and the necessity for plotting the readings.

MR. T. D. LYNCH (*by letter*).—Referring to Mr. Howard's discussion, I wish to call attention to his statement that "the point of departure of the curve from a straight line will be its proportional limit, or preferably it may be designated as its elastic limit, the term under which it has so long been known." Again, he stated that "the term elastic limit inherently carries with it a definite meaning." Mr. Lynch.

Unfortunately, the term elastic limit is not being generally used to mean the point of departure of a curve from a straight line, nor is it being used with a very definite meaning. On the contrary, the term elastic limit has come to be used to mean the same to some engineers as the proportional limit, to other engineers the same as the yield point and to many other engineers it may mean any indeterminate point on the curve between

[1] *London Engineering*, December 12, 1913, p. 799; "On the Measurement of Stress by Thermal Methods," *Transactions* of the Royal Society of Edinburgh, Vol. XLI, Part 2, No. 9; "The Relation of Thermal Change to Tension and Compression Stress," *Transactions* of the Royal Society of Canada, Second Series, 1904–1905, Vol. X, Section III.

Mr. Lynch. the proportional limit and the yield point. The existence of this fact is the reason for so much discussion on this subject.

Let us hope that the definition for proportional limit, elastic limit and yield point as recommended to the Society for adoption may result in the proper use of these terms by its members, thus establishing standards that will be followed by all testing engineers, designing engineers and others who may have occasion to use these terms.

Mr. Malysheff. MR. A. P. MALYSHEFF (*by letter*).—It is well known that the determination of yield point on the testing machine has been given preference in practice to that of limit of proportionality. The American Society for Testing Materials, realizing the consequences of the confusion of these two terms, brought

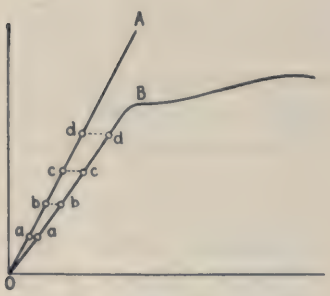

FIG 1.

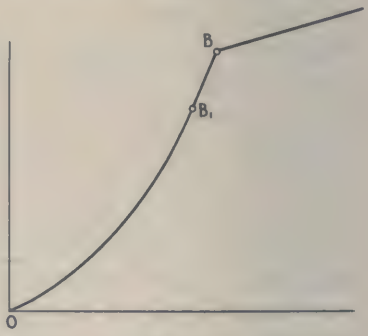

FIG 2.

up, at the annual meeting in June, 1916, the question of the relation between them. Summing up this discussion, we arrive at the following conclusions: (1) The determination of limit of proportionality may be definitely obtained, but the determination of yield point depends upon the testing engineer (J. E. Howard); (2) the yield point is, however, higher on the curve than the limit of proportionality; and (3) the difference between the two may in some instances be as high as 90 per cent (experiments of T. D. Lynch). It is possible that this difference may even exceed this figure.

Notwithstanding these facts, it is probable that testing engineers will continue to determine the indefinite yield point rather than the more definite limit of proportionality until a

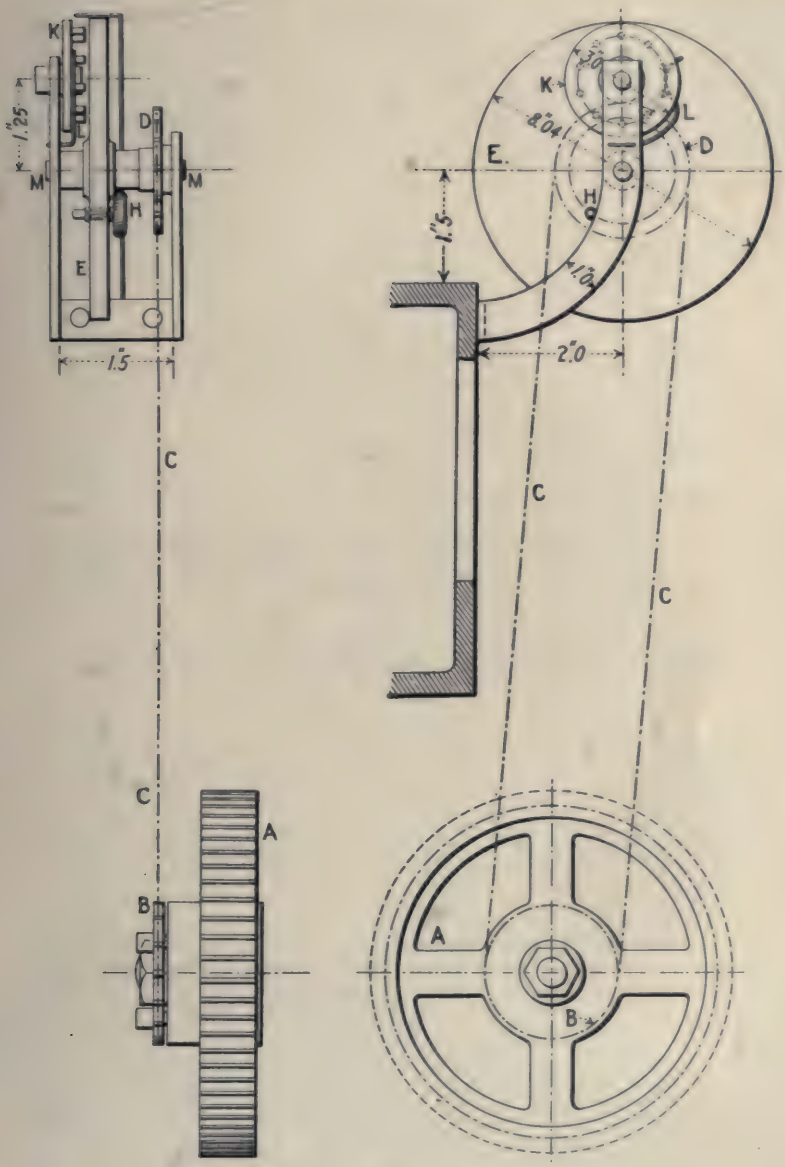

FIG. 3.

Mr. Malysheff. comparatively simple method has been discovered for the determination of the latter. The method described below was adopted by the writer in his work at his testing laboratory at the Eddystone Ammunition Corporation during the past seven months, and he considers it satisfactory for quick results and sufficiently

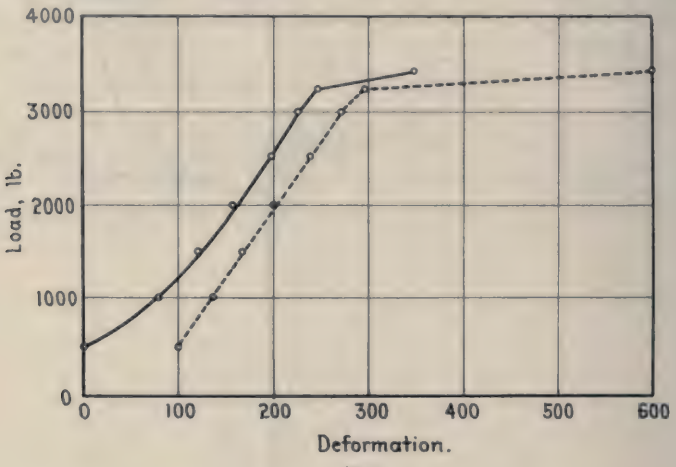

FIG. 4.

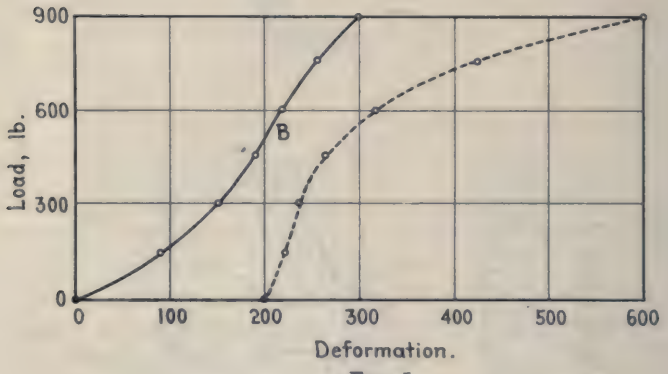

FIG. 5.

exact for practical purposes. Taking into consideration the fact that the absolute differences between the periodical readings on the extensometer are necessary only when the modulus of elasticity is being determined, in the case of the determination of limit of proportionality each of the readings can be increased

at the same rate or at proportionally increasing rates without interfering with the indication of limit of proportionality. Bearing this in mind we can simultaneously make observations on the deformation of the specimen together with that of the testing machine itself, considering the latter as being within the limit of proportionality. The deformation of the testing machine is represented by the straight line *O A*, Fig. 1. If the deforma- Mr. Malysheff.

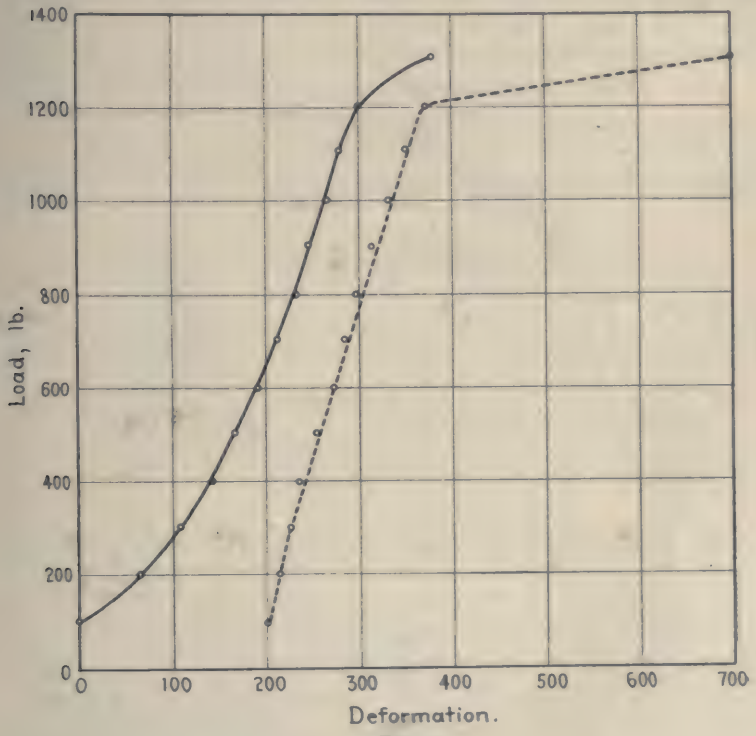

FIG. 6.

tions of the specimen at the different points are marked by the lines *a a*, *b b*, *c c*, etc., there will be no confusion in the determination of limit of proportionality by taking the combined observations. Point *B* may be easily determined. On account of clearances in the moving parts of the testing machine which do not disappear immediately when the machine is started, the theoretically straight line *O A* will in practice not be straight

Mr. Malysheff. but will have a form as represented in Fig. 2. At the beginning of the test the deformations are quite large. Later they decrease and finally have a tendency to be constant at the part B_1 B. At the point B the limit of proportionality is reached; this point is shown as clearly as by the use of the extensometer. In order to utilize the method described various devices may be applied, depending upon the type of the testing machine. It is, however, advisable to mount the device on such parts of the machine

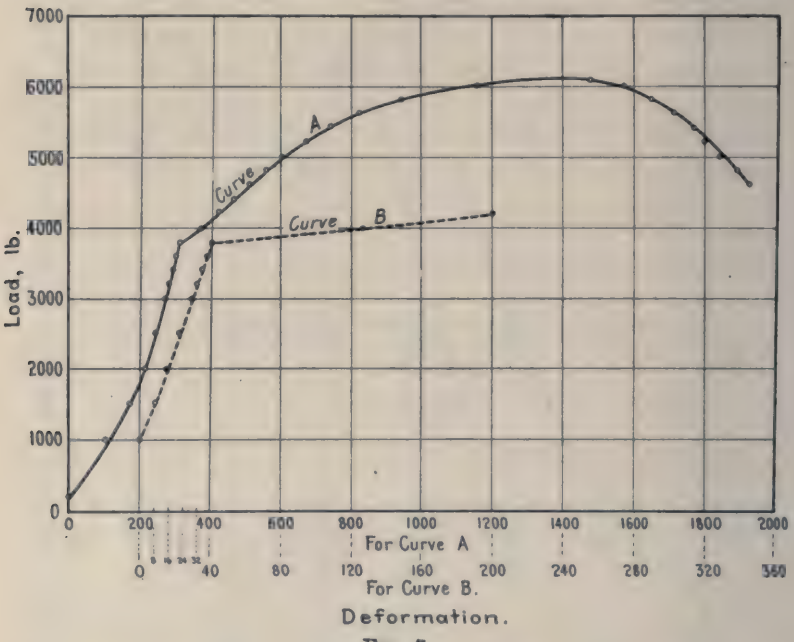

FIG. 7.

as are most convenient for the operator, and preferably in such a manner as to minimize the influences of the clearances of the moving parts of the machine.

The device suggested was adapted by the writer for an Olsen testing machine, and is illustrated in Fig. 3. On the wheel A of the testing machine a chain wheel B was mounted and connected with chain wheel D by means of an endless chain C C. Chain wheel D and disk E are mounted on shaft M M. The circumference of disk E is divided into one hundred equal parts. Disk

E is provided with a set screw H which connects or disconnects disk E with small wheel K by being screwed in or out. When disk E completes one revolution the wheel K, whose circumference is divided into ten equal parts, will turn one-tenth of a revolution, that is, one division. A spring L prevents wheel K from revolving when not connected with disk E. The dimensions of the apparatus have been selected so as to render the deformations of the specimen plainly visible. When the testing machine is run at high speed it is recommended to unscrew the set screw H, thus disconnecting disk E and wheel K.

The writer has made a number of tests, noting simultaneously the readings of the device and of the extensometer. Figs. 4, 5, 6 and 7 are examples of these tests. Fig. 4 shows

FIG. 8.

FIG. 9.

the result of a test of 0.45-per-cent carbon steel. The full line represents the readings of the device and the dotted line the readings of the extensometer. The dimensions of the test section of the specimen were: diameter, 0.25 in., and length 2.5 in. The limit of proportionality can readily be seen. Fig. 5 shows a test of aluminum. Taking account of the fact that the beginning of the permanent deformation is shown by the change in direction of the curve at the point B, we may say that even in the case of aluminum that moment is indicated simultaneously. Fig. 6 shows the result of a test of brass. The dimensions of the test section of the specimen were: diameter, 0.35 in., and length 3.5 in. The device continues in action until the time of rupture of the specimen and allows observations to be made

Mr. Malysheff. not only while the load increases but even after it has reached its maximum (Fig. 7). On all figures points of observation are marked with small circles.

In order to obtain the best results the ends of the specimen should be threaded (Fig. 8), or the testing machine provided with special holders (Fig. 9). When the specimen is gripped in the testing machine by means of wedges, the device indicates not only the deformations of the testing machine and of the specimen but also the sliding of the wedges: this condition renders the real rate of limit of proportionality difficult to obtain. The device is equally adapted for specimens thin or thick, long or short; and may be used to equal advantage for tension, compression and bending tests. The economy of time by the use of this device in comparison with the extensometer is considerable, as the necessity of marking the specimen and adjusting the extensometer is avoided. About a thousand tests were made with the device at low speed, and the average time necessary for each test was found to be about four minutes, exclusive of the time taken for measuring the specimen.

SOME EXPERIMENTS ON THE PLASTIC ELONGATION OF WIRE.

By A. V. de Forest.

SUMMARY.

This paper attempts to account for the phenomena of "creep" or slow stretch of wire under load, and subsequent self-hardening in the light of an interpretation of the amorphous cement theory of Beilby.

Results are given of tension tests on bronze and brass wire at temperatures from 20 to 185° C., which exhibit the points under discussion.

A description of the apparatus is added. The tension test machine is of the dead-load type, using a water load, and recording the stress-deformation diagram on photographic paper.

SOME EXPERIMENTS ON THE PLASTIC ELONGATION OF WIRE.

By A. V. de Forest.

In the course of tension tests on wire, using a dead-load method, it was observed that brass wire stretched very differently from copper, aluminum, nickel, soft iron and German silver. These latter stretched in a fairly uniform manner, that is, the stress-strain diagram showed an apparently smooth curve, while that for brass was an irregular one. Turning to the literature it was found that this phenomenon is clearly shown in Dalby's autographic stress-strain diagrams, and is evidently the same effect as the well-known "self-hardening" of bronze, and certain aluminum alloys when tested in the usual manner. The amorphous cement theory, advanced by Beilby and extended by Rosenhain, seems to offer a simple explanation of this matter, and further experimental work was undertaken to support the theoretical reasoning.

As is universally known, the amorphous theory accounts for the hardening effect of cold work by assuming that a layer of amorphous material, sub-microscopic in thickness, is formed on the cleavage planes, and between grains where slip has occurred. This material is assumed to be of the nature of an under-cooled liquid, such as glass, stiff and brittle. The effect of this intergranular layer is to "cement into rigidity" the enclosed blocks of crystallin material. After the metal has been deformed, it may thus be regarded as composed of broken parts of the original grains, in the form of crystallin blocks, surrounded and supported by the amorphous cement. The manner in which the metal yields to a deforming force will then depend on the relative resisting power of the two components.

There is little direct evidence on this point, but what seems a simple idea of the matter will account for the observed facts. At room temperature the cement formed between crystallin blocks of some metals requires a considerable time in which to

reach its maximum supporting power. Muir's experiments on mild steel show that several days are required before overstrain tension specimens reach their greatest elastic limit. Howe has likewise shown that iron and mild steel behave in a similar way with respect to hardness. Using a word readily suggested by the term "cement," it would seem that the amorphous layer required a given time in which to set. This time might be expected to be dependent on the temperature, whatever the nature of the setting process; whether or not this process consists in the recrystallization into the adjacent crystallin blocks of the surplus amorphous material, leaving a layer of the same order of magnitude as that filling the original intergranular spaces. In fact both the above experimenters found the time required for setting was reduced from a few days to a few minutes by heating the metal to 100° C. Whether the properties of the crystallin component are changed by such a small change in temperature is unknown, but it seems reasonable to suppose that the effect of such change on the unstable cement would be greater than that on the stable crystallin matter. Even the unstable forms of hardened steel are apparently little changed by heating to 200° C. As the facts can be readily explained without assuming such a change, it will be omitted for the sake of simplicity in the present case.

The setting process is then probably largely a function of the temperature. It may likewise be supposed that the viscosity of this substance is also affected by smaller changes in temperature than those which sensibly affect the crystallin particles. The rapid elongation under constant or diminishing loads found by many workers at higher temperatures, and the great intergranular brittleness found by Rosenhain at temperatures close to the melting point, indicate a great change in viscosity.

From these considerations it would seem that a metal subjected to a gradually increasing dead load would stretch smoothly and uniformly, when the rate of stretch and the temperature were such that the cement would set comparatively slowly. The blocks of crystallin material moving along the slip planes would be supported by an amorphous layer so viscous that no rapid motion would be possible. The motion of the blocks would be gradually checked by the increasing area of cement, and its

increasing stiffness as time was allowed for it to set. The retardation would be gradual and the end of the motion would be imperceptible. Under these conditions the stress-strain diagram would be a smooth curve, such as is usually obtained from copper.

As the temperature is raised the cement between the blocks of crystallin matter becomes less viscous at its formation, and therefore the slip becomes more rapid. Also the time required

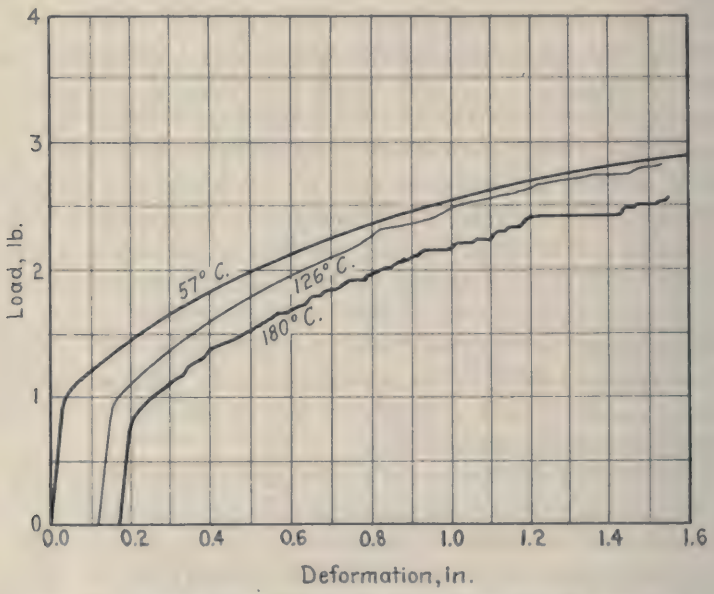

FIG. 1.—Stress-Strain Diagrams for Annealed Phosphor-Bronze Wire. Time of Loading for each Test, about 3½ min.

for setting is less, and the relative motion of the blocks is more suddenly arrested. As the deformation increases, slip along new planes begins, only to stop suddenly when the resulting cement stiffens in its turn. A much slower rate of loading might also result in the same form of irregular stress-strain diagram, for the mobile phase formed by any one slipping movement would have time to yield to a perceptible degree before the load became sufficient to develop slip along new planes.

At annealing temperatures the existence of the amorphous material would seem to be limited to that in the intercrystallin spaces, for any formed between planes of crystallin matter of the same orientation would probably be instantly reabsorbed by the growing crystal. There would then be practically no viscous yielding of amorphous material between moving blocks, and it seems probable that the slips would be so minute as to integrate into continuous motion, showing a smooth curve for

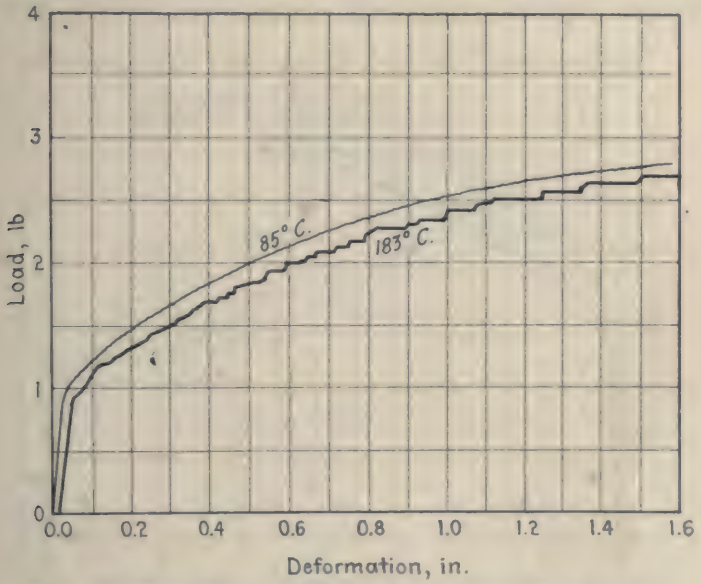

Fig. 2.—Stress-Strain Diagrams for Annealed Phosphor-Bronze Wire. Time of Loading for each Test, about 16½ min.

the stress-strain diagram, as at low temperatures. At high temperatures, just below the melting point, the amorphous cement is perhaps so fluid as to allow the crystal grains to fall apart under very small stresses. This has been demonstrated by Rosenhain.

In the stress-strain diagram the setting of the cement should be registered by an increase of strength shown by a break in the curve toward the vertical, provided the viscosity of the amorphous material in its mobile state is not sufficient to

mask this effect. When a tension test is carried out slowly enough, the viscous yielding of the material is noticed in an ordinary testing machine as "creep," and the setting of the cement as "self-hardening." In the curves, creep is shown as a break toward the horizontal, and self-hardening by a change in direction toward the vertical. Some of these effects are clearly shown by the results of tension tests on wires.

Fig. 1 represents autographic stress-strain diagrams from tests on phosphor-bronze wire loaded at a uniform rate. It

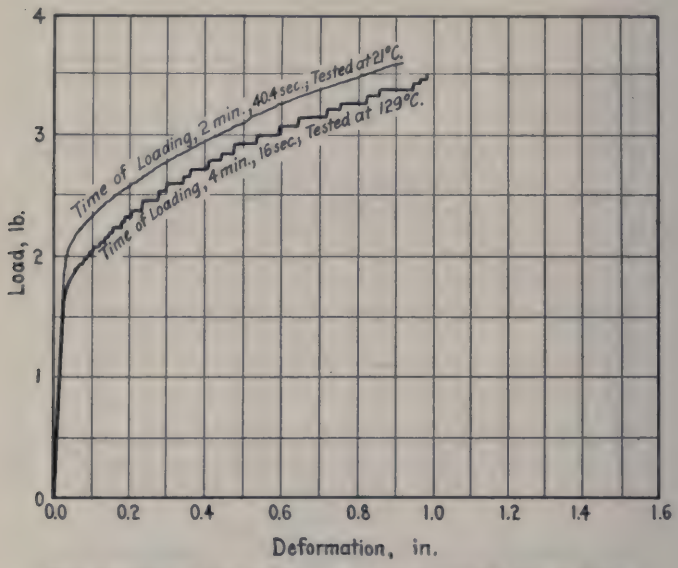

FIG. 3.—Stress-Strain Diagrams for Annealed Brass Wire.

will be seen that at 57° C. the curve is smooth, as it is at room temperature; at 126° C. there are wavy irregularities; and at 180° C., the irregular curve is broken by horizontal lines representing very rapid slips. The rate of loading is the same in each case.

Fig. 2 shows curves from similar wires at a slower rate of loading. At 85° C. the curve has begun to show slight irregularities and at 183° C. it is practically a broken line.

In wires of this composition the viscosity of the cement is so great that there is no increase in strength at the moment of

set until the temperature reaches about 60° C. At 126° C. there is an intermediate condition, and at 180° C. the viscosity is so small as to allow the wire to stretch rapidly with but little resistance until the setting of the cement suddenly stiffens the metal, and a considerable increase in load is necessary to again produce an elongation.

Fig. 3, from tests on brass wire, shows the same effect.

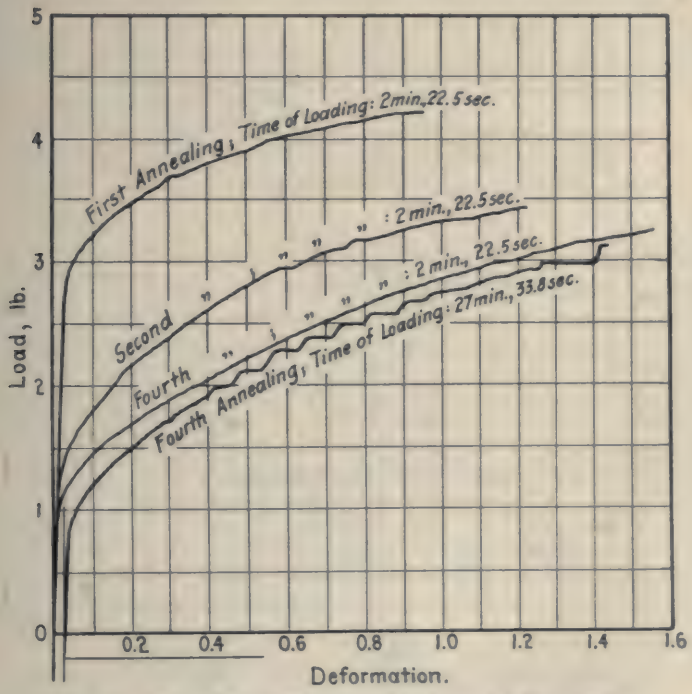

FIG. 4.—Stress-Strain Diagrams for Annealed Brass Wire.

At room temperature this wire gives a curve showing slight irregularities. A lower temperature would undoubtedly give a smooth curve. At 129° C. the curve is a series of straight lines. Here the viscosity of the cement is so small, and recovery so complete that after each stretch the metal has hardened into a state exactly similar to its condition below the elastic limit. Each vertical portion of the curve is therefore exactly parallel to the portion below the elastic limit.

In Fig. 4, the curves for material tested after fourth annealing show the same effect, but it is produced solely by a change in

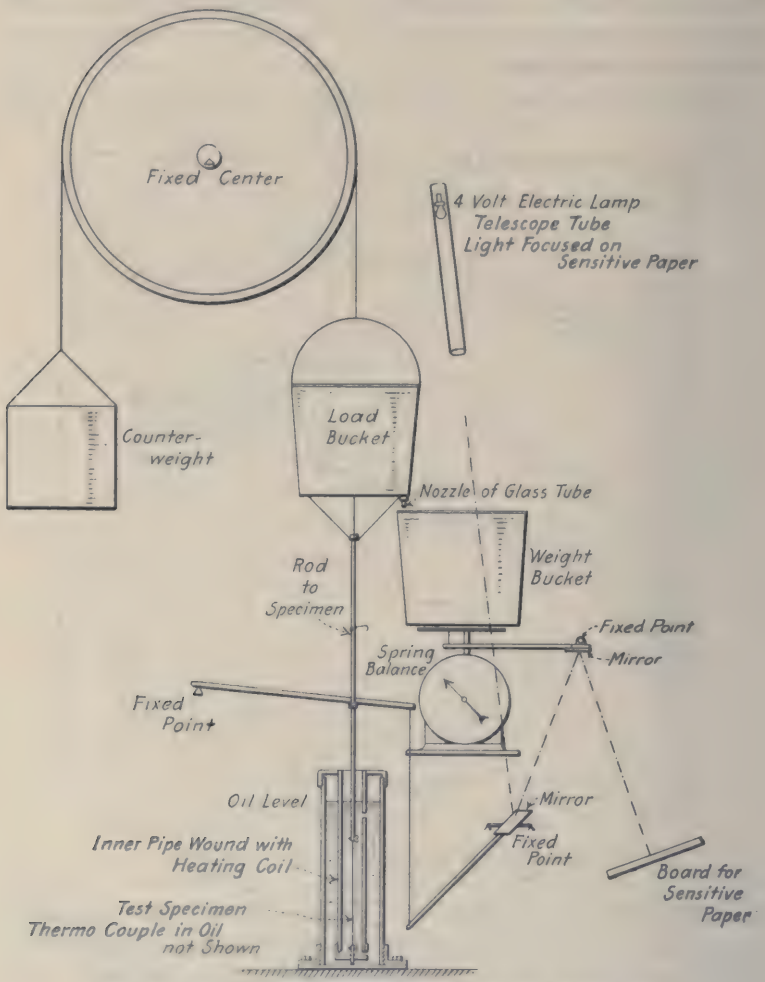

FIG. 5.—Apparatus for Drawing Stress-Strain Diagrams.

the rate of loading. With rapid loading the cement has not time in which to yield before new slip planes are developed by the rapidly increasing load. When loaded slowly,—in 27 min-

utes,—the cement has opportunity to yield and set firmly before the increasing load again produces a slip.

Owing to the limitations of the apparatus, it was impossible to prove that in the above cases the stress-strain diagram would again become smooth at higher temperatures, but the work of Rosenhain and Humfrey; of Pisati, as quoted by Huntington; and the experiments on brass by Bengough and Hudson, indicate that the irregular stretching at intermediate temperatures is followed by regular elongation at higher temperatures. The last two papers report the production of sound, perhaps while in the region of irregular stretch.

The remaining curves of Fig. 4 show the effect of the temperature of annealing on the brass wire used in the experiments.

The form of apparatus which was adopted after several trials is shown in Fig. 5. A large pulley is supported by a frame work, on one side of which hangs a bucket of water, balanced by a counterweight on the other. Below the bucket is an oil bath with electric heater, at the bottom of which one end of the specimen is fastened. The other is secured to the bottom of the bucket and pulled up, the weight on the wire being exactly the weight of water which was allowed to flow out of the bucket through a nozzle in the bottom. A spot of light reflected from a mirror displaced by the motion of the bucket, and from a second mirror moving proportionally to the weight of water caught in a receptacle placed on a spring balance, drew the stress-strain diagram on a piece of bromide paper. Annealing was done in a small resistance furnace, and temperatures measured with a thermocouple and galvanometer.

Bibliography.

Bengough, *Journal*, Institute of Metals, Vol. VII, No. 1, p. 166 (1912).

Bengough and Hudson, *Journal*, Institute of Metals, Vol. IV, No. 2, p. 92 (1910).

Dalby, *Proceedings*, Royal Society, Vol. 88*A*, p. 281 (1913).

Howe, "Metallography of Steel and Cast Iron," p. 457 (1916).

Huntington, *Journal*, Institute of Metals, Vol. XIII, No. 1, p. 50 (1915).

Parker, *Journal*, Institute of Metals, Vol. II, No. 2, p. 215 (1909).

Rosenhain, "Introduction to Physical Metallurgy, pp. 241-256 (1915).

Rosenhain and Humfrey, *Journal*, Iron and Steel Institute, Vol. 87, No. 1, p. 219 (1913).

DISCUSSION.

MR. HENRY M. HOWE.—Mr. de Forest shows us that the smoothness of the stress-strain diagram of two copper alloys is destroyed by moderate heating, say to 180° C., and that that of one of them is destroyed by slow loading, the smoothness being replaced by a jaggedness which increases with the temperature at which the straining occurs. Howard has shown such jagged stress-strain curves for steel.[1] **Mr. Howe.**

The author refers the roughening of the diagram to abruptness in the rate of "setting," and hence of spontaneous strengthening of the amorphous metal supposed by Beilby to form along the slip planes on which, according to Ewing and Rosenhain, the actual travel of the minute blocks of crystallin metal occurs during plastic deformation.

Before plastic deformation begins, for instance in a cast or in an annealed metallic mass, substantially all of the metal is crystallin. When plastic deformation starts, according to our present view, certain parts of this crystallin metal along certain microscopic planes pass into a mobile state, and so act as a viscous lubricant and enable the crystallin blocks on either side of them to slip past each other. From this mobile state the metal passes progressively into a rigid vitreous state, to which the greater hardness of cold-worked metal is referred. It is, indeed, the temporary mobile state of this amorphous metal that permits plastic deformation. As this deformation proceeds, fresh portions of the crystallin metal enter the mobile state, thus permitting the crystallin particles on either side of them to slip past each other, and pass on to the rigid amorphous state, so that at any given instant there is a continuous procession of an almost infinite number of metallic particles from the initial

[1] "Report on Tests of Metals at the Watertown Arsenal," 1888, p. 248, and folders Nos. 13, 14, and 17. His steels of 0.20, 0.31 and 0.57 per cent of carbon gave these jagged diagrams at 225°, 106°, and 157° C. respectively. The 0.20-per-cent steel also gave progressively less rough diagrams at higher temperatures, 353° and 415°, and the 0.31-per-cent carbon steel at 298° and 516°, but at both higher and lower temperatures all three steels gave the usual smooth curves. The rate of straining was such that from 5 to 10 minutes were occupied by each test.

Mr. Howe. crystallin through the mobile into the final rigid state. At any given instant during the deformation certain particles have already passed beyond the mobile into the rigid state; others have passed only as far as the mobile state; while still others remain in their initial crystallin state. Deformation is replaced by rupture when the residual supply of mobilizable crystallin metal has fallen so low that it can no longer be mobilized fast enough by the deformation to heal the cracks which the deformation itself is causing in the already rigid amorphous metal, and in the crystallin metal itself.

The drop at the yield point in the stress-deformation diagram, on this view, reflects the weakening of the test piece as a whole by two agencies: first by the passage of much metal from the crystallin into the weak mobile state, and second, by the decrease of sectional area which results from the permanent stretch. The arrest of this drop means that the strengthening of the test piece as a whole, caused by the passage from the weak mobile state to the strong final amorphous state, is now equalling both these simultaneous weakening agencies; the rise of the diagram means that this strengthening is now exceeding the simultaneous weakening; and the drop at the maximum load means that the generation of the strong amorphous metal no longer suffices to offset these two weakening agencies, and especially the weakening effect of the decrease of sectional area. That, at least, is my interpretation.

The slips by means of which the little crystallin blocks pass each other during plastic deformation are so minute that the largest of them thus far observed under the most favorable conditions is of the order of size of a wave of light. Their minuteness and the habitual smoothness of the stress-deformation diagram agree with the conception that each measurably large permanent elongation of the test piece represents the integration of an inconceivably enormous number of these inconceivably minute slips, and that at any given instant enormous numbers of individual lots of metal must be in each of the innumerable steps of the progress from crystallinity through mobility to rigidity; numberless lots just beginning to mobilize; numberless others in the full flush of mobility; numberless others in each of the many imaginable stages between the maximum mobility and

the final maximum rigidity. At present I cannot reconcile this conception with the author's stepped curves. Mr. Howe.

The author's conception one fancies to be somewhat different: that at any given instant the metal which has thus far mobilized is, if not wholly, at least in very large part, in substantially one and the same stage in this crystallin-mobile-rigid series, or perhaps better that the mobilization occurs, not so progressively as I have conceived, but in rather large gushes, so that at any given instant that which has already left the crystallin stage but has not yet become rigid is mostly in about the same stage of mobility; that the stepping of the diagram results from a lack of correspondence between the rate of weakening through mobilization and the rate of strengthening through setting; that the mechanism through which raising the temperature brings about the saw-toothing of the diagram is first by hastening the weakening by leading to greater initial fluidity and hence greater weakness of the incipient stage of mobility, and second by hastening the later stage of strengthening by hastening the rate at which the mobile metal sets, so that we have a set of jerks, first a jerk of weakening as a lot of metal suddenly mobilizes and next a jerk of strengthening as this mobilized metal suddenly sets, then a new weakening jerk, and so forth.

The difficulty which I find in this type of explanation is that it does not fit in well with our general observation that the diagram is normally smooth, and saw-toothing a rare peculiarity. For if the mobilization and resetting were thus in relatively large individual gushes, then smoothness of the diagram should occur only when the rate of mobilization of these rather large gushes matched their later rate of setting so nicely that their resultant effect failed to roughen the diagram. Such close matching should then be the great exception and saw-toothing of various forms and degrees should be the rule.

An additional difficulty in the way of the author's explanation is that his supposed hastening of the setting of the mobile metal by gentle heating, inferred from the hastening of the hardening cause by such heating (Muir and others), though true of iron, has not been shown to be true of the alloys with which he deals. On the contrary, the elaborate investigation of

Mr. Howe. Mathewson and Phillips shows only an extremely slight hardening on heating cold-rolled brass to 200° C.[1]

If I find the author's explanation of the influence of temperature hard to follow, I must confess far greater difficulty in following his explanation of the effect of rate of straining.

I have ventured to lay these difficulties before you, because a frank statement of one's inability to explain phenomena so often incites another to explain them clearly. In any event we are indebted to the author for his very interesting discovery, which is likely to help in matching up this picture puzzle, as every additional datum should.

Mr. de Forest. MR. A. V. DE FOREST.—Mr. Howe has very kindly given a clear and comprehensive view of the plastic deformation of a metal. I also have to thank him for a better exposition of my own views than I have been able to express.

Mr. Howe finds that the stress-strain diagram is normally smooth. It seems to me that very few curves are made under circumstances which would show irregularities in any case. The ordinary type of testing machine is not suitable, for the inertia of the beam prevents continuous exact balance. A method, using a weigh-bar, such as Professor Dalby's, with a photographic record, is necessary to obtain an accurate diagram. Most testing work has been done at atmospheric temperature and at convenient rates of loading. Systematic investigation of the effect of varying these factors independently would perhaps show unsuspected roughness. A delicate means of detecting irregularities is to measure the *rate* of elongation with a constant rate of loading. A sensitive apparatus of this kind has shown small but definite irregularities in the stretch of copper, nickel, iron and aluminum where the stress-strain diagram was apparently smooth. It may be that all deformation proceeds by jerks, but most of the jerks have escaped detection through being small and unlooked for, with the exception of the striking yield point of mild steel.

The great increase in size of the jerks under certain conditions indicates, to my mind, that the rate of loading and the condition of the amorphous cement are then such that a slipping movement once started along certain innumerable slip planes,

[1] *Bulletin*, Am. Inst. Mining Engrs., January, 1916, p. 6.

proceeds further than it would if the cement were more viscous, and that this over-running of the slipping surfaces, combined with the more rapid setting of the cement which accompanies lack of viscosity, produces instantaneous stiffening of the stretching zone in the wire. This effect of over-stretching the material along the planes of deformation would be accentuated by the inertia of the moving weight in the dead-load type of apparatus, and perhaps makes this form better suited to showing up irregularities. Mr. de Forest.

The saw-tooth appearance of the stress-strain curve is then caused by a certain relation between rate of loading and condition of the cement. A slow rate of load might allow a cement, too stiff to flow appreciably at rapid loading, time in which to deform measurably before new slip planes were started by the increasing load. The necessary variation is probably experimentally impossible in many cases, but the influence of the rate of load at high temperatures has been noted by most workers.

This explanation, as Mr. Howe points out, is difficult to reconcile with the known minuteness of slip-planes, and theorizing would be easier were all deformation curves smooth, yet the facts seem unfortunately to demand a theory which will account for saw-toothing.

Mr. Howe observes that Mathewson and Phillips found but slight hardening of cold-rolled brass at 200° C. It is not stated in their report that any attempt was made to determine a rate of increase in hardness after rolling. It may be that in some metals, the rise in temperature due to the rolling operation, as ordinarily carried on, is sufficient to fully harden the intergranular cement. In any event, measurements of this factor are greatly to be desired in order to extend our knowledge of an intricate subject.

CONSTANTS AND DIAGRAMS FOR REPEATED-STRESS CALCULATIONS.

By H. F. Moore and F. B. Seely.

SUMMARY.

This paper is supplementary to the paper presented by the writers at the annual meeting of the Society in 1915 entitled, "The Failure of Materials under Repeated Stress." A revised table of constants is given, based on further study of test data, and on the correction of a numerical error discovered in the method of deriving values of the constants given in the paper presented in 1915.

Diagrams are given for the solution of the exponential equations noted in the previous paper, and a discussion is presented of the "factor of safety" as applied to repeated-stress conditions.

CONSTANTS AND DIAGRAMS FOR REPEATED-STRESS CALCULATIONS.

By H. F. Moore and F. B. Seely.

At the 1915 annual meeting of this Society the writers presented a paper entitled "The Failure of Materials under Repeated Stress."[1] In that paper the following formulas were proposed for failure of metals under repeated stress:

$$S = \frac{B}{(1-Q)N^{\frac{1}{8}}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

or $$\log S = \log B - \log (1-Q) - \tfrac{1}{8} \log N \ldots\ldots\ldots (1a)$$

and $$S = \frac{B}{(1-Q)N^{\frac{1}{8}}} (1+0.015\, N^{\frac{1}{2}}) \ldots\ldots\ldots\ldots (2)$$

or $$\log S = \log B - \log (1-Q) - \tfrac{1}{8} \log N + \log (1+0.015\, N^{\frac{1}{2}}) \ldots (2a)$$

In these formulas S is the intensity of fiber stress in pounds per square inch (computed by the ordinary formulas of mechanics of materials) corresponding to failure after N repetitions of stress; Q is the ratio of minimum stress during one cycle of stress repetition to the maximum stress during the cycle; B is a constant determined experimentally for any metal. Equation (1) was recommended for use in designing members whose failure would endanger life or limb; equation (2) for cases in which danger to life or limb is not involved.

Tentative values of the constant B were determined from a study of test data, and were given in the paper above referred to. Since the presentation of that paper further study of test data has been made, and an error has been discovered in the method of computing B from test data noted therein.[2] The correct method of determining B from data of tests in which $Q = -1$ (stress completely reversed) is as follows:

[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part II, p. 437 (1915).

[2] The following is quoted from the paper of last year: "B has been determined for a number of materials from the data of repeated stress tests. It is the value of the ordinate for $N = 1$ (line extended backward) and $Q = -1$ (stress completely reversed)."

On a logarithmic diagram of test results in which values of S for failure are plotted as ordinates and values of N as abscissas, the value of B is *twice* that of the ordinate for $N = 1$ (line extended backward). After correcting the error in the paper and making further study of test data, tentative values of B are submitted, as given in Table I.

For solving the complex equations for repeated stress given above, the writers present two diagrams (Figs. 1 and 2), which they have found more convenient to use than the diagrams given in the previous paper.[1] Fig. 1 gives the diagram corresponding

TABLE I.—TENTATIVE VALUES OF B.

Material.	B	Log B.
Structural Steel and Soft Machinery Steel	250 000	5.39794
Wrought Iron	250 000	5.39794
Steel, 0.45 per cent Carbon	350 000	5.54407
Cold-rolled Steel Shafting	400 000	5.60206
Tempered Spring Steel	400 000 to 800 000	5.60206 to 5.90309
Hard-Steel Wire	600 000	5.77815
Gray Cast Iron	100 000	5.00000
Cast Aluminum	80 000	4.90309
Hard-drawn Copper	140 000	5.14613

to equation (1) and Fig. 2, the diagram corresponding to equation (2). The method of using either diagram is as follows:

Enter the diagram at the lower edge with the desired value of N as abscissa; pass vertically to the diagonal line for the value of B for the given material (if the exact value of B for the material is not plotted the location of its line can be judged by interpolation with a good degree of accuracy); then pass horizontally to the diagonal line for the value of Q corresponding to the given range of stress; then vertically to the upper edge of the diagram where the value of S for fatigue failure may be read from the scale. It should always be kept in mind that in

[1] Attention is called to the alignment charts for the solution of equation (2) given by Prof. J. B. Kommers, of the University of Wisconsin, in the *American Machinist* for Feb. 3, 1916.

no case is S to be taken greater than the static strength of the material.

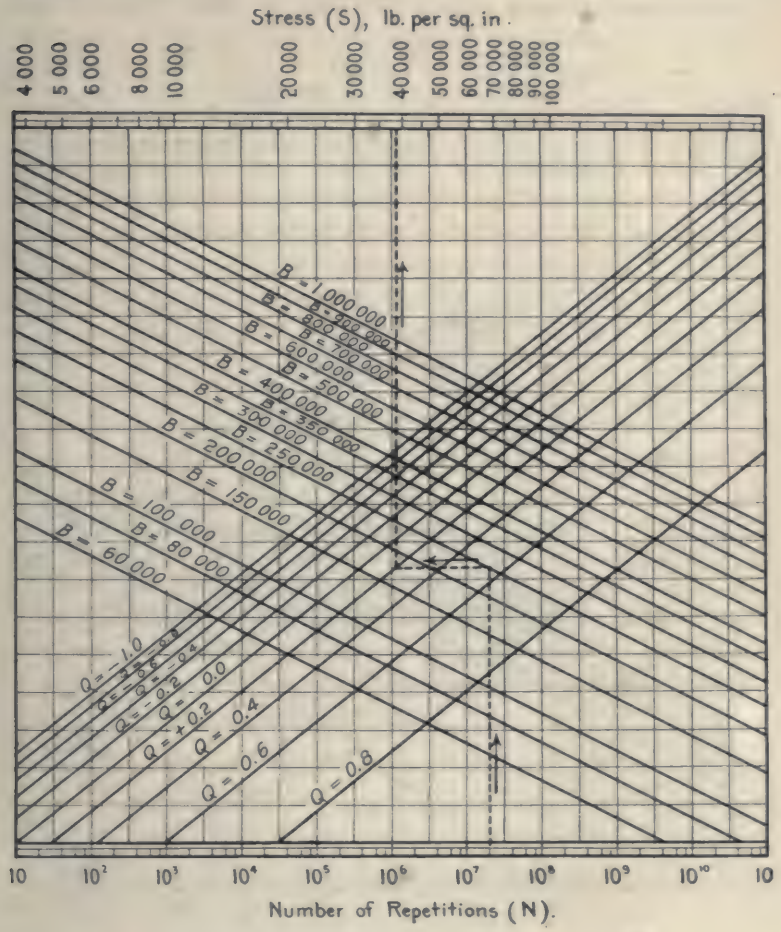

FIG. 1.—Diagram for Solution of Equation (1), $S = \frac{B}{(1-Q)N^{\frac{1}{6}}}$.

Example:

In Fig. 1 the dotted lines indicate the solution of the following problem: Value of B for the material (structural steel), 250,000; $Q = +0.2$; $N = 20{,}000{,}000$; find S. *Ans.*—$S = 38{,}000$ lb. per sq. in. and static strength would govern.

In Fig. 2 the dotted lines indicate the solution of the following problem: Value of B, 400,000; $Q = -1.0$; $N = 12{,}000{,}000{,}000$; find S. *Ans.*—$S =$ 14,400 lb. sq. in.

In studying repeated-stress test data, or in choosing working stresses for members subjected to repeated stress, it should be remembered that a small reduction in stress very greatly

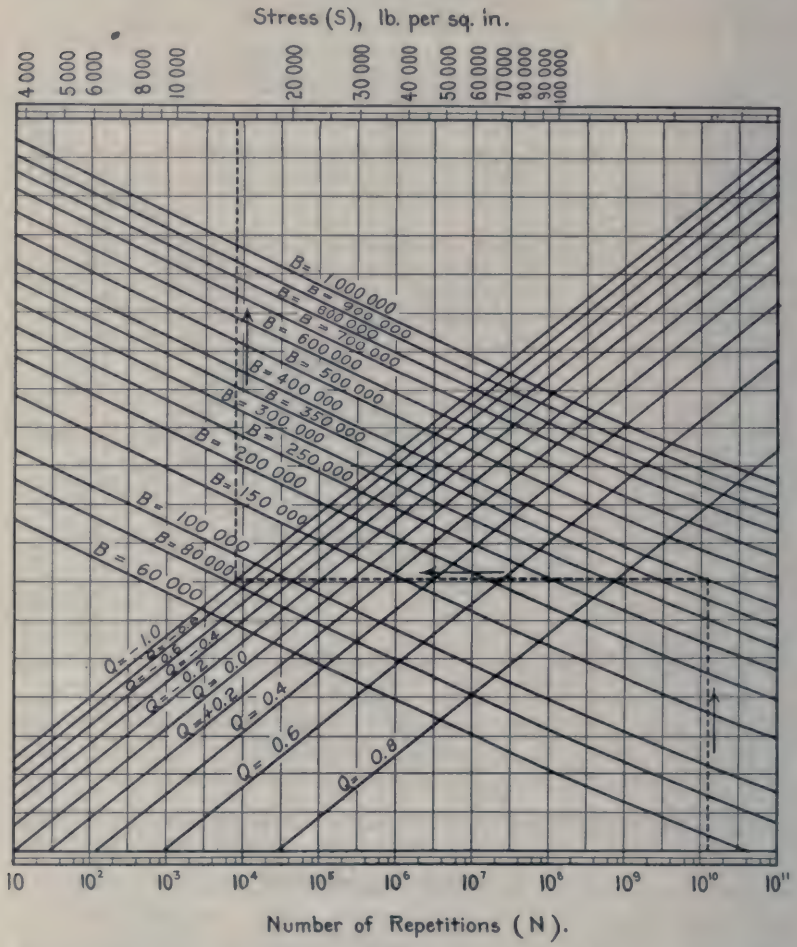

FIG. 2.—Diagram for Solution of Equation (2), $S = \frac{B}{(1-Q)N^{\frac{1}{9}}}(1+0.015N^{\frac{1}{9}})$.

increases the number of repetitions of stress which the member can carry. For metals a decrease of 9 per cent in stress about doubles the endurance, so far as can be determined from test data. Since the number of repetitions is so sensitive to changes

in the magnitude of stress, it seems more logical for repeated-stress problems to apply the "factor of safety" used to the number of repetitions rather than to the stress, computing the probable stress at failure for a number of repetitions many times greater than the number which the member is expected to withstand in service. If the factor of safety is applied to N, then it should be very much larger than the factors commonly applied to stresses in static-stress problems. Our test data for repeated stress is very much less extensive than our test data for static strength, and the test results show wide variation in N for small change in S; and while the equations given yield results a little lower than the average results of tests, yet for some few tests failure occurred at lower values of N than the equations would indicate. To guard against this variation, and against the large variation in N which would be caused by slight variations in the stress actually applied to the material, a factor of safety of 100 would not seem too large, if applied to the number of repetitions of stress.

In problems involving static strength it is customary to apply the factor of safety to the stress which will cause failure. If this practice is followed for repeated-stress problems (remembering always that under any conditions the stress allowed must not be greater than the safe *static* working stress for the material) a factor of safety of 1.8 would correspond to an increase of endurance of something over 100 times.

NATIONAL STANDARD SPECIFICATIONS AND THEIR RELATION TO EXPORT TRADE.

By William R. Webster.

SUMMARY.

The recent agreement by which the U. S. Department of Commerce will translate and distribute abroad many of the standard specifications of the American Society for Testing Materials, is one of the highest compliments that could have been paid the Society. This naturally calls for every assistance that the members can render to ensure the success of this important movement, by bringing these specifications into more general use on export orders.

Before foreign engineers will consider the adoption of these specifications for orders to be placed in this country, they will want to know how they were arrived at by the Society, whether they are in general use here and how they compare with the specifications of the British Engineering Standards Committee. The writer has therefore endeavored to give full information on these subjects, from his long experience as Chairman of Committee A-1 on Standard Specifications for Steel. He has also cited instances of the difficulties met in enforcing some of the requirements of foreign specifications from his own practice of over eighteen years as consulting and inspecting engineer involving the use of foreign specifications on export orders. In conclusion he has indicated the advantages to be derived from the use of the A. S. T. M. standard specifications for export orders, and has recommended a definite plan for keeping these specifications abreast of the best engineering practice.

NATIONAL STANDARD SPECIFICATIONS AND THEIR RELATION TO EXPORT TRADE.

By William R. Webster.

General Considerations.—The success of the American Society for Testing Materials is largely due to its successful efforts in the standardization of specifications for iron, steel and other materials of construction. This work was very much simplified and put on a common-sense working basis from the start by giving the manufacturers of materials equal representation and vote with the engineers representing the consumers on all committees dealing with specifications. As the importance of this work became known to the engineers of the country, the membership of the committees had to be materially increased from time to time. After several years some of the principal committees found it desirable to put their specifications in the hands of separate sub-committees in order that matters pertaining to each specification might receive prompt attention. These sub-committees report to their parent committees and these report in turn to the Society at the annual meetings. This work has now become thoroughly systematized and is carried out in a manner that leaves little to be desired. Most of the arduous detail work falls, of course, on the members of the sub-committees, to whom the Society is under great obligation for their time and efforts, not to mention their outlay for traveling expenses, etc., which the Society has been unable to assume.

The Society may be said to act as a clearing house in relation to specifications, having helped very materially towards reducing the number of specifications in extensive use. In cooperation with other societies it has also done much in the way of harmonizing its specifications with theirs, and many railroads have adopted these specifications at their individual initiative or through their associations.

There is, of course, a legitimate field for other specifications, as, for example, for materials for special structures, or where

unusually severe conditions of service have to be met; or again, where an engineer from his own personal experience feels justified in insisting on special tests or unusual requirements. Some of these requirements may seem at first sight unnecessary and unreasonable to the manufacturers, but after giving them a fair trial, it is often found that they serve as a good check on the quality of the material, and that they can be complied with more easily than was anticipated. In such cases they gradually come into general use, and are embodied in our specifications.

Government Cooperation.—The Society is very fortunate in having officers of the Army and Navy on both its main and sub-committees. They have taken an active interest in this work, and have been of material assistance. Our standard specifications have been used extensively in government circles, notably in the purchase of materials for the Panama Canal. These specifications are representative of the best general practice in this country. They are widely used and have the same standing in this country as have the specifications of the British Engineering Standards Committee in England.

While the Society has never received any direct appropriations from the government for its research work it has been helped in various other ways; for instance, the U. S. Bureau of Standards has rendered valuable assistance by putting its experts, laboratories, and testing facilities at the disposal of various committees without charge. In this way matters of general importance suggested by representatives of consuming and manufacturing interests in joint conference are submitted to an impartial and competent body of men for investigation. The reports are based on facts as found, and the committees are left to draw their own conclusions, the Bureau of Standards making no rulings in matters of this kind.

Furthermore this Bureau has done a great deal of excellent work towards standardizing methods in chemical determinations and in the testing of materials and has conducted investigations in relation to protective coatings, cement, non-ferrous metals, textile fabrics, etc. The members of this Bureau have also contributed many valuable papers to the Proceedings of the Society, containing the results of their researches in the general field of its activities.

The Society has received similar support from other departments of the government service, notably the Bureau of Chemistry, the Bureau of Forestry, the Bureau of Mines, and the Office of Public Roads and Rural Engineering.

Investigations at Watertown Arsenal.—In 1908 Genl. William Crozier, Chief of the Bureau of Ordnance, U. S. A., offered the use of the large Emery Testing Machine and heavy machinery at Watertown Arsenal free of all expense to a body of engineers representing prominent manufacturers and consumers of structural materials for research work, the appropriations to that Bureau having been materially increased to admit of such work. This led to an important meeting in New York attended by the Commanding Officer, Watertown Arsenal, and some forty engineers. After fully discussing the great opportunity thus presented for getting much needed reliable data on engineering subjects of a kind that could only be obtained through government assistance, on account of the large expense involved for materials, shop work, testing, etc., two committees were appointed to formulate definite programs for the proposed investigations. With a view of keeping in close touch with the officers of the arsenal, who became ex-officio members of these committees, and with the work, it was decided to hold future meetings at Watertown.

To one committee was assigned the design, fabrication and testing of full-size steel columns up to the capacity of the large testing machine, as very few large steel columns had been tested up to that time, and reliable data for column formulas were greatly needed. The other committee was to investigate the structure of open-hearth and Bessemer steel ingots, blooms, billets, and finished steel as manufactured under the conditions of every-day practice. This material was to be cut up longitudinally and transversely at every stage of manufacture and to be examined for segregation, piping and other internal defects. Records were to be kept of the internal structure of the steel at different passes in rolling from the ingot to the finished material. Similar records were to be kept of the external defects, due to ingot cracks, tearing of the surface in blooming and overlaps in rolling, etc.

With this information before them the committee was to

have several lots of similar ingots cast, and finished material of the same section rolled under conditions purposely arranged so as to increase each kind of internal and external defects, in order to ascertain if possible the true cause of defects of each class.

The next step in the committee's work based on the results thus obtained was to have other lots of ingots cast and finished material rolled under conditions designed to minimize or eliminate these internal and external defects. The committee expected to get in this way enough reliable data to enable them to make definite recommendations as to modifications in the manufacture and rolling of steel calculated to insure better material without great reduction of output. This work was to be done practically under the auspices of the American Society for Testing Materials, as the members of this committee were almost without exception members of that Society. It was in fact expected to utilize the results of this investigation in the revision of the A. S. T. M. steel specifications, both as to manufacture and check tests on the finished product, all with a view of ensuring better material. Unfortunately, before the first series of tests had been completed, this investigation was brought to a sudden close by an Act of Congress, and research work transferred to the Bureau of Standards. They have continued some parts of the investigation on the lines suggested, so far as their funds and facilities permitted, but no attempt has as yet been made to complete the work as outlined by the original Watertown Committee. This is especially regrettable, since the work mapped out would have been the most complete series of tests and experiments that had ever been undertaken in this important field. Had it been carried to a conclusion, some of the problems that are troubling us to-day would doubtless have been solved, the government would now be securing better steel for its guns and the railway companies better steel rails.

The work had been carried far enough, however, to show the great necessity of casting sound ingots, as many more internal defects were found in the ingots, blooms and finished materials than had been expected. A full set of plates containing the photographs of all the sections of this material was published by the government in Tests of Metals, Volumes I and II, 1909. This caused several investigators both here

and abroad to start experiments with different methods of casting ingots to overcome defects which were shown not to be confined to any particular locality. Excellent results have been obtained and no doubt some of these methods will soon come into more general use both in England and in this country.

The committee also expected to determine through their proposed second and third series of tests, what improvements might be made in the structure of the steel by heat treatment in rolling, that is by making the proper reductions at the proper temperatures, and by avoiding too high finishing temperatures. When the change of structure that can be accomplished by proper heat treatment in rolling is better understood, a further investigation will be in order to determine the necessary changes in our rail sections to admit of rolling the steel at temperatures best suited to give the much desired fine-grain structure in the finished rail.

At the Twenty-fourth Annual Convention of the National Association of Railway Commissioners held in 1912 its Committee on Rails and Equipment made a very comprehensive report, in which they referred to this investigation and recommended that it be continued along the general lines originally planned, and that special study be given to rail steel made by the basic open-hearth process.

Recommendations.—Enough has been said to show the importance of completing the investigation outlined by the committee, taking into account all recent improvements in the manufacture of ingots and additional tests embodied in our own and other specifications, designed to check the soundness of the steel. It is not too much to expect that arrangements can be made with the Bureau of Standards to take up this important work with a proper committee, and that assistance can be secured from other government departments by which the use of their heavy machinery will be obtained for cutting up the large ingots and rolled material for internal examination.

The writer would accordingly recommend that the Society take whatever steps may be necessary to secure the cooperation of the Bureau of Standards in this work, and that the members of the original Watertown Committee be invited to become

members of any new committee that may be appointed to carry on such investigations.

International Specifications.—While the International Association for Testing Materials undertook in 1895, at its organization in Zurich, the standardization of methods of testing, and the introduction of international specifications, nothing was actually accomplished in the matter of international specifications until the atmosphere had been very much cleared up by the work of the standards committees in various countries. The conflicting requirements on export orders, the trouble they gave, and the success of the American Society for Testing Materials in overcoming similar difficulties on home orders by the introduction of standard national specifications led the International Association to undertake the standardization and introduction of international specifications for steel. It was finally decided to consider for each class and from each country, materials of only one standard specification prepared by an equal number of representatives of both the manufacturing and consuming interests. The following specifications were agreed to as representative national specifications:

English specifications published by the Engineering Standards Committee, London.

German specifications recognized by the Deutscher Verband für die Materialprufungen der Technik.

American specifications adopted by the American Society for Testing Materials.

The manufacturers of each country were asked to become familiar with the standard specifications of each of the other two countries, and to work to them whenever they were called upon to do so. It was expected that in this way the manufacturers would gradually become accustomed to the points of difference in the three standard specifications, and that these differences might then be harmonized and a single specification agreed upon for each class of steel, the International Association for Testing Materials thus becoming a clearing house for international specifications.

This plan was, however, rejected on the claim that it gave the same value to the steel from each country although manu-

factured under different specifications. It was fully explained by the American members of the committee that:

> "The time has passed when any specification or set of specifications with clauses calling for low phosphorus requirements based on favorable ore conditions, would tend to confine orders to any one locality or country inasmuch as the basic and duplex processes have equalized matters in this respect so that there is now no commercial advantage to be gained by insisting on specifications of this kind. Such matters can now be settled by the committee on their intrinsic merits, without reference to their commercial bearing."

After considerable delay and discussion it was proposed to start with a single specification that could be agreed upon for each class of material, and for this purpose the adoption of Lloyd's specifications for ship material with the addition of chemical requirements for sulfur and phosphorus was suggested for adoption as an international specification.

Objections were raised to these chemical requirements, and although it was shown that they were in accordance with the standard practice in this country, this had no effect whatever, and these requirements had to be withdrawn. It was then claimed that Lloyd's specifications were already recognized as international specifications, and that their adoption by the Association would not give them any added weight. Although it was frankly admitted that these specifications were satisfactory in every respect, there was an unwillingness towards their adoption as international specifications. Lloyd's specifications for ship material were thus rejected, and this practically put an end to the whole matter of international specifications for steel after several years of useless work on the part of subcommittees in each of the three countries, who did not at first appreciate that they were really working at cross purposes.

The International Railway Congress Association at its Congress in Berne in 1910 discussed the advantages of international specifications for iron and steel, and passed resolutions showing its great interest in the work at that time and its desire for information as to progress by the International Association for Testing Materials.

Unfortunately there has been little or nothing to report since then, as the action of the International Association for Testing Materials at its New York Congress in 1912 practically put an end to international specifications before anything had been accomplished. A number of our own standard specifications were translated into foreign languages for the use of the members of that congress. The Secretary of the International Association for Testing Materials, when asked to send copies of these specifications to the members of the International Railway Congress Association in view of its request for such information as shown by the above-mentioned resolutions, or to authorize the American Society for Testing Materials to distribute them, replied that if the English and Germans should translate their specifications into other languages and make the same request, the Council would authorize such distribution.

Specifications in Foreign Languages.—Fortunately some of our steel manufacturers and locomotive builders, etc., distributed these specifications through their representatives abroad, and found that when the foreign engineers had these specifications before them in their own languages it was of great assistance in securing orders based on our specifications. This was the unanimous report of those who followed this course and showed clearly that the best course for this Society to follow is to improve its specifications wherever possible, and to avail itself of any proper opportunity to bring these specifications to the notice of engineers in foreign countries, and thus to make them available for use on export orders.

The manufacturers also stated that the material supplied was practically the same as it would have been under the foreign specifications, but that it was much simpler to manufacture under the conditions with which they were familiar; also that they have had most excellent reports from this material in service.

The Society is to be congratulated that Secretary Redfield of the Department of Commerce has become interested in this matter and has seen the great advantages that would accrue to the manufacturers of this country by following such a course. He has therefore agreed to translate and publish many of our specifications into Spanish, French and Portuguese. Each

specification will have the original English version attached to it, and all numeric quantities will be given in both the British and the metric system. The Department of Commerce will also distribute these specifications among engineers abroad through its own staff and the United States consular offices. The Society has been very much complimented by this recognition of the value of its specifications on the part of the Department of Commerce, and will doubtless cooperate in every possible way towards ensuring the success of this important undertaking.

Further Recommendations.—Since our specifications differ in some respects from those in general use abroad, a special committee of this Society should be appointed to which all matters connected with differences with foreign specifications might be referred for explanation or investigation. In this way prompt action would be secured, and any matters of sufficient importance might be referred to the standing committee which has that particular class of material in charge.

It is not reasonable to expect that foreign engineers who have had satisfactory results in service from materials manufactured in this country under their own specifications will consider placing orders here under our specifications unless they can thus secure more prompt delivery. Nevertheless, the specifications should be sent them so as to be available when needed.

Variations in Conditions in Different Countries.—It is natural that any country having iron ores low in phosphorus and sulfur will make very strong claims in favor of the use of acid open-hearth steel in preference to basic open-hearth steel. But in a country like our own where both acid and basic open-hearth steels are made in large quantities and are used side by side for most purposes, it is rather hard to understand the decided prejudice in England against basic open-hearth steel. On looking into this matter we find that it is partially due to their former experiences with this steel when the operations in the manufacture of basic steel were not so well understood as at present, and to the prejudice that has always existed in that country against basic Bessemer steel. It has been shown, too, that basic open-hearth steel made from very high

phosphorus iron is not as reliable as that made from the lower phosphorus iron used in this country.

There is also decided objection in England to steel made by the duplex process, although we are getting excellent results in this country from steel made by this process. On investigation we find that in England the basic-Bessemer process is used at the start, and is followed by the open-hearth process. No doubt some of the highly oxidized basic Bessemer metal is carried through the open-hearth furnace to the finished product and this would account for the objections to this steel.

In this country the metal is first blown in the acid Bessemer converter, and is not as highly oxidized as that from the basic Bessemer converter, which doubtless accounts for the better results obtained from the steel manufactured by the former method. In ordinary American practice the duplex-process steel is now supplied on orders calling for basic open-hearth steel, without making any distinction whatever, whereas on export orders it has always been necessary to explain carefully the differences between the processes of manufacture in this country and abroad, when it is desired to use duplex steel on orders calling for basic open-hearth steel.

Difficulties with Foreign Specifications.—Further variations are found between foreign specifications and ours in the matter of drop tests on rails. These have given considerable trouble owing to the great variation in the conditions under which such tests are made here and abroad. For instance, the weight of the tup varies from 2000 to 2400 lbs.; the distance between supports from 3 ft. to 3 ft. 6 in. and 4 ft.; and the length of the test rail may be 6 ft. or 30 ft. The matter is complicated much further by the different weights of the anvils used, and the character of their foundations, by which the effectiveness of the blow of the falling weight is so largely affected. In this country the standard drop testing machine is in general use at all rail mills. This has also been adopted by the American Society for Testing Materials, and its use is called for in the standard specifications of the Society.

Some foreign specifications prescribe a drop test of 30 ft.

for rails 30 ft. long. For rails of the same weight other specifications require an 18-ft. drop test for rails 6 ft. long.

In order to find out just what this difference in the requirements meant, a series of comparative tests were made, and it was shown that the long overhanging ends of the 30-ft. rails act as cantilevers, supporting the central part, and that the 18-ft. drop test is more severe on a piece of rail 6 ft. long, than the 30-ft. drop test on a 30-ft. piece of rail.

Another complication in the use of foreign specifications on export orders is that often minor points of difference cannot be quickly settled. The responsible engineers are usually far away, and there is not time to refer questions to them for decision. In cases where the specified chemical requirements do not agree with the physical requirements, the inspector is sometimes forced to reject material that he knows from his experience to be satisfactory. It may even give better physical results than those specified, but may fail to meet all the specified chemical requirements. Much of the existing confusion is no doubt due to the general change from acid open-hearth to basic open-hearth steel, since a higher carbon content is required in the latter for the same ultimate strength. This is better understood now than formerly when some of these specifications were first drawn up.

These differences are only a few of those which have been encountered in working to foreign specifications, but they suffice to show the advantages that would be derived from the use of national standard specifications, such as those of the American Society for Testing Materials.

A careful study of foreign specifications will bring to our attention requirements and proof tests that are not in general use here. These should be thoroughly investigated, and in so far as they may be found to afford valuable additional checks on the quality of the material, they would doubtless be adopted in our own specifications. In this way we may hope gradually to secure national standard specifications that will embody the best features of the foreign specifications, and become the nearest approach to international specifications that we can expect to obtain.

DISCUSSION.

Mr. Moldenke. MR. RICHARD MOLDENKE (*by letter*).—As one of the members of the Society who is fully conversant with the policy of the International Association for Testing Materials in regard to international specifications for export purposes, and holding diametrically opposite views on this point from those expressed in Mr. Webster's paper, I beg to offer the text of the New York Congress resolution mentioned by him, as also the resolutions adopted by the Council of the International Association at their last meeting in Turin. The members of the Society may draw their own conclusions as to the correctness of Mr. Webster's views on the subject.

Resolution adopted at the New York Congress of the International Association for Testing Materials, September 7, 1912:

"In view of the present difficulties attending the preparation of international specifications, the Congress recommends to the Council that the reference to Committees 1 (*a*) and 1 (*b*) for the continuation of their work be the collection and dissemination of information in regard to the modifications in the specifications of the different countries; and that these committees be instructed to present their report to the Council from time to time and await further instructions as to proceeding with the preparation of international specifications."

In view of the foregoing the International Committee 1 (*b*) on Cast Iron and Cast Iron Products continued its activity and presented three specifications for international export use to the attention of the Council of the International Association.[1]

The following action was taken by the Council of the International Association for Testing Materials:

Resolution adopted by the Council of the International Association for Testing Materials at a meeting held in Turin, Italy, April 4, 1914:

"The Council of the International Association for Testing Materials welcomes the preparation of standard specifications for the delivery of materials of construction intended for international trading purposes.

"The Council fully realizes the difficulties attending any attempt to unify existing specifications of the countries adhering to the association, but

[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XIV, Part I, p. 149 (1914).

sees in the agreement upon standard specifications for export purposes, a distinct advance in international efficiency and comity. Mr. Moldenke.

"The Council asks a full discussion and trial of the standard specifications so far presented, so that, if possible, final action be taken at the St. Petersburg Congress. The Council also hopes to receive further proposals, and requests that each country may stimulate the industries towards early action in the direction of standard specifications for international trading purposes, in conjunction with the other producing countries."

Apart from whatever effect the racial animosities engendered during the horrible war may have on the subject, and the, by this time, absolutely negligible chance for the adoption of the standard specifications of any country, our own included, by any of the contesting nations, it seems to me that we should do everything possible to further ultimate international specifications for export in which every nation interested shall have had a hand. The other neutral nations will help us; but we are in a position to take the lead, and the International Association for Testing Materials is the logical and only available means to the end.

In view of the fact that International Committee 1 (*b*) expects to resume the good work immediately upon the close of the war, the American members of that committee wish the position of the International Association, as shown by the above-mentioned resolutions, to be thoroughly understood by the members of the American Society.

MR. WILLIAM R. WEBSTER (*Author's closure by letter*).— Mr. Webster. Dr. A. V. Rieppel, Chairman of International Committee 1 and Sub-committees 1 (*a*) and 1 (*b*), in the following letter of June 17, 1913, to the members of those sub-committees, disposed of any remaining chances there may have been at that time for agreement on, or adoption of, international specifications for steel by the International Association for Testing Materials:

NÜRNBERG, JUNE 17, 1913.

TO THE MEMBERS OF SUB-COMMITTEES 1 (*a*) AND 1 (*b*).
Chairmanship of Committees.

Dear Sir:

In my last report, which you have already received, and in a Resolution of Congress in New York in 1912, it was mentioned that my efforts, covering a long number of years, to introduce standard international specifications for

Mr. Webster. iron and steel had unfortunately not led up to any tangible result and reasons were given for this state of affairs. So long as there is such diversity in the conditions of production in force in the chief producing countries, there seems to me to be no prospect of arriving at an international standardization of the specifications. There does not seem, however, to be any likelihood of an alteration effecting this preliminary change being introduced.

In view of these circumstances and of other important calls upon my time, I regret to state that I have been obliged to tender to the I.A.T.M. my resignation of the Chairmanship of Committee 1 and Sub-committees 1 (*a*) and 1 (*b*).

I beg of you to take note of the above.

I have pleasure in taking this opportunity of expressing my sincerest thanks to all the Members of Committee for enabling the task to be approached at all by their zealous and disinterested collaboration.

I beg to remain,

Yours very truly,

A. V. RIEPPEL.

To MR. WILLIAM R. WEBSTER.

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